OWN AND SPILLOVER EFFECTS OF A CONDITIONAL CASH TRANSFER PROGRAM TARGETING YOUNG GIRLS: EVIDENCE FROM INDIA

A Dissertation

Presented to

The Faculty of the Department

of Economics

University of Houston

In Partial Fulfillment

Of the Requirements for the Degree of

Doctor of Philosophy

By

Sakshi Jain

May 2018

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Abstract

My dissertation evaluates both own and spillover effects of a conditional cash transfer program targeting young girls. The first chapter evaluates the effects of an ongoing conditional cash transfer (CCT) program in India targeting young girls that provides incentives in the form of deferred cash payments, with eligibility at birth and the largest payment coming at age 21. Using a difference-in-differences framework, I exploit variation in exposure to LLY by birth cohort, state, and birth order to estimate the causal impact of the program on childrens health and educational outcomes, as well as the fertility behavior of parents. I find that the program significantly increased the likelihood of registering the births of daughters. However, the sex ratio at birth does not change, suggesting no change in female mortality. I find evidence of families having children faster and in turn reducing the birth spacing. However, parents are moving toward lower eventual family sizes as I find an increase in the likelihood of parents adopting sterilization. Finally, the results using math and reading test score data show some evidence of improvement in the education outcomes of daughters. Overall, I find that a financial incentive program plays a limited role in affecting the well-being of girls.

The second chapter paper evaluates the spill-over effects of an ongoing conditional cash transfer (CCT) program in India on the ineligible older siblings living in the same households as the eligible beneficiary. Using difference-in-differences framework, I exploit variation in exposure to LLY by birth cohort, state, and birth order to estimate the spillover effect of the program on the ineligible older sibling's educational outcomes. I find that the program does not have any effect on the schooling outcomes of the ineligible siblings. I also find negative effect on the math and reading skills of the ineligible siblings of treated children relative to the children in families that received no treatment. The evidence suggests that this effect operates through the reallocation of resources away from the ineligible sibling towards the eligible sibling.

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to Papa

Contents

List of Figures

List of Tables

Chapter 1

Effects of a Conditional Cash Transfer Program Targeting Young Girls: Evidence from India

1.1 Introduction

India has been struggling with a widening gender gap in most socio-economic indicators and the federal and state governments are making efforts to bridge these gaps. Programs and policies have been introduced with the goal of improving the status of girls, particularly in terms of infant survival, health, and education. The government of Madhya Pradesh (MP), one of the poorest states in India, introduced such a program to empower young girls.[1] The program, called Ladli Laxmi Yojana (LLY), was launched and announced in 2007 and is the first conditional cash transfer (CCT) program in the state.

Beginning with Progresa in Mexico, CCT programs have been widely used to increase school enrollment and improve health outcomes of poor children. CCT programs

[1] Government of India:http://pib.nic.in/archieve/lreleng/lyr2001/rjun2001/20062001/r200620011.html

typically target school-aged children in poor households. Eligible families then receive cash payments conditional on children's school attendance and (or) regular health check-ups. In brief, these programs induce the poor to invest in human capital development of their children without constraining their budget allocations. Earlier literature (Schultz, 2001; Skoufias and di Maro, 2006; Behrman and Parker, 2011) shows evidence of mostly positive impacts of CCT programs on school-aged children. These programs have been shown to have positive short-run effects on poverty and long-run effects on human capital development.

Although, there is a large literature of studies evaluating the conditional cash transfer (CCT) programs, analysis of this particular program is of interest because it differs in design from traditional CCT programs like Progresa, on several dimensions. First, the program covers only first or second born girls, and from the time of birth. This is important in terms of addressing the issues of infant mortality and prenatal sex selective abortions. Second, the payments are deferred. They are made in installments starting from enrollment in grade sixth,followed by payments made at meeting eventual schooling targets, and the biggest payment comes at age 21. This raises the issue of how households with liquidity constraints may respond to the program . The financial incentives consist of a longterm savings bond redeemable on the daughters 21st birthday conditional on her staying unmarried until the age of 18 and completing school level education. Additionally, the parents (either or both) have to adopt sterilization at the time of enrolling their second (birth order) daughter. These dimensions of variability in the design allows me to exploit variation by birth cohort, state, and birth order, which was not possible in previous studies.

This paper is the first to provide evidence on the effects of Ladli Laxmi Yojana in India. I assess the impact of LLY on improving the skewed sex ratio, changing family size and composition, and education outcomes of girls. I estimate the intent-to-treat program effects on sex ratio among living children, fertility preferences, and sterilization.

In order to understand how the parental investments have changed, I look at the effect of the program on eligible daughters health and schooling. Using the official state panel (Civil Registration System Data) and individual level data from nationally representative household surveys (India Human Development Survey and Annual Status of Education of Education Report), I exploit variation in exposure across birth years, states, and birth order to estimate the effects of LLY. Employing a difference-in-differences framework, I compare eligible female children in Madhya Pradesh to their ineligible counterparts in MP and five other similar states. I find that the Ladli Laxmi Yojana had a positive effect on female registration at the time of birth, but find no significant effect on sex ratio at birth or at later-life survival. There is an increase in the number of parents getting sterilized after the program, but surprisingly not after the second daughter, as designed under the policy. However, there is limited evidence of parents increasing investments in daughters health and education in the short and medium term, as a result of the program.

The main challenge for identification of this program's impact is that it was launched at the same time across the entire state of Madhya Pradesh. Also, the launch of the scheme was at a time when Madhya Pradesh was experiencing high growth, and plausibly increasing rates of return to education as a result. Thus, changes in girls' health and education outcomes during this period, could simply be because of the high growth in the state.

I address this concern by using variation in the composition of eligible children as well as the state and the year of birth to estimate the effects of the program. Following the estimation in Duflo (2001), I treat older cohorts who were not exposed to the program when they were born as the comparison group and younger cohorts who were exposed to the program in Madhya Pradesh as the treated group. Then use the other five states, Chattisgarh, Jharkhand, Orissa, Rajasthan and Uttarakhand as comparison states. Finally, I add third and later born children, who were ineligible for the program, as another comparison group.

The key dependent variables of interest are sex ratio, family size compositions, parents' sterilization, and schooling outcomes. First, I employ a difference-in-differences (DID) estimation by comparing these outcomes across first and second born girls in treated and untreated cohorts in Madhya Pradesh and comparison states. However, the difference-in-differences estimate will only give the causal impact of the policy, as long as the parallel trends assumption holds. Since I reject the parallel trends assumption, I employ a triple difference (DDD) approach to estimate the effects of the program. In addition, using difference-in-differences, the threats of omitted variable concerns still remain, including economic growth and changes in investments in school and hospital quality. The triple difference framework help address most of these issues. Therefore, I compare the changes in outcomes of first and second born girls to the changes in outcomes of third and later born girls. Given that third and later born children were not eligible to participate in the program, I use them as a comparison group to control for possible omitted variables at the state-year level.[2] Moreover, most of the third born children in the comparison group are not actually in families where the first and second born daughters are eligible.[3]. To the extent that there are state-specific shifts in policies, programs or spending for health and education, the higher birth order children will help control for these state-specific time effects. For example, third born children born in 2006 and 2007 cohorts will certainly not be in families that would have been eligible for the policy, i.e., those with first or second born girls in 2006 or later. Therefore, the third born girls are from the same birth cohorts as the first and second born girls, but are never eligible either before or after the policy because of their birth order. Therefore, I compare the DID estimate with the estimate for the third and higher birth ordered

[2]Even though the program affects the family size, my results on changes in family size show that most people continue to have three children. This makes using the third and later born children as a valid comparison group.

[3]Given that I am studying the effects up to year 2011-12 and up to 2009, five and three years after the program, respectively for sex ratio and schooling outcomes

females in treated and control states. This triple difference is the preferred estimate of program impact on sex ratio and schooling outcomes.

My main findings are as follows: First, I find no evidence of a change in either the sex ratio at birth or girls' later-life survival. However, registered births for girls increase significantly. This seems to be a response to the program condition of registering the daughter within the first year of birth. This led to an increase in sex ratio of registered birth by almost 2%, enough to eliminate the imbalance in sex ratio.[4] I find that the likelihood of parents getting sterilized goes up by 8 percentage points, and by 15 percentage points among parents with eligible children. The response towards the adoption of sterilization and smaller family sizes is driven by the non-BPL households.[5] I also find a positive effect on the schooling outcomes of eligible girls, and also an improvement in their math and reading skills. It is useful to note that although the girls are still quite young at the time of measurement, I already find evidence of an improvement in their education outcomes, both in terms of schooling and test scores.

Although LLY was implemented in only one Indian state, it is representative of a set of more recent programs introduced in other states along several dimensions. Since, the program is being touted as one of the most successful programs to save and empower women in the state, other states are also emulating and introducing such programs. Therefore, it is important to understand the effects of a financial incentive program with enrollment at birth but delayed future payout. Furthermore, understanding its strengths and weaknesses, as highlighted by my analyses, can help improve the design of programs with similar socio-economics characteristics.

The rest of this paper is organized as follows. Section 2 discusses the related literature; section 3 describes the program; section 4 describes the data; section 5 discusses estimating equations, and identification strategy; section 6 presents the main results;

[4]The natural rate of sex ratio is 105 males per 100 females.
$http : //www.searo.who.int/entity/health\_situation\_trends/data/chi/sex - ratio/en/$

[5]BPL are the Below Poverty Line households. The government issues a BPL card to families whose income falls below the poverty line.

section 7 shows robustness checks and section 8 concludes.

1.2 Related Literature

Evaluation of most CCT programs in South America and some South Asian countries show that these programs effectively increase poor households' investments in the development of their children's human capital. There is a vast literature examining the effects of CCT programs on children's health and education outcomes (Skoufias et. al, 1999; Skoufias, Davis, and De La Vega, 2001; Behrman, Sengupta, and Todd, 2005; de Janvry et. al, 2006; Chaudhury and Parajuli, 2010; Burde and Leigh, 2013). Most of these studies find a positive effective of financial incentive programs on improving health and education outcomes of poor children.

Over the years, the issue of Indias missing women (Sen, 1990), has attracted a lot of attention amongst policymakers. Studies claim that about half a million girls go "missing" every year in India (Jha et al 2006). While the exact figure continues to be a debated issue, it is clear India is still struggling with the problem of skewed sex ratio and gender gaps. The sex ratio at birth has declined sharply and is still far from the natural sex ratio at birth of 952 girls to 1,000 boys. A substantial proportion of the decline in the female share may be attributed to discrimination against girls in the form of non-registration of female birth, sex selection, infanticide and parental neglect.

Indian federal and state governments have introduced several financial incentive programs to improve female survival, and health and education investments in girls. However, there is limited evidence on the effectiveness and causal effect of these financial incentive programs on change in parental investments and attitudes towards their daughters. Anukriti (2017) evaluates one such financial incentive program in India targeted at reducing fertility and improving sex ratio. She finds that the program, called Devirupak, increases the probability of couples having just one son. Moreover, the program does not change parents attitudes towards having daughters, even though the incentives given to parents with just daughters, were higher. Sinha and Yoong (2009) evaluate the effects of another such program in Haryana, Apni Beti Apna Dhan, a financial incentive program aimed at reducing the sex ratio. They find a positive effect of the program on sex ratio but no change in desired fertility.

This paper adds to the existing literature on the effects of a CCT program in a number of important ways. First, my paper quantifies the effect of a CCT program that is quite different in design from the traditional CCT programs. This is important because most CCT programs are for poor households and begin payments from the time of school enrollment. Additionally, most financial incentive programs in India, are targeted just to below poverty households (Apni Beti Apna Dhan) or do not have any future financial benefits (Devirupak), both in terms of savings bonds or conditional on schooling outcomes. LLY on the other hand has the biggest payment far out in the future but enrollment in the program begins at the time of birth. Second, while many studies have focused on South America and other regions of the developing world, this is the one of the few studies to focus on evaluating the impact of CCT programs in India. India is a transition economy with 17% of the worlds population, but is still grappling with the problem of skewed sex ratio and strong son-preferences with huge gender gaps in health and education. Thus, it is useful to understand how such programs affect the well-being of children in the country. Third, only a handful of studies have examined the effect of CCT programs on changes in fertility and family sizes; most of the previous studies have focused on educational and health attainment. There is a large literature on the impact of quantity on quality (Becker and Lewis, 1973; Rosenzweig and Wolpin, 1980; Rosenzweig and Schultz, 1987; Pitt, Rosenzweig and Gibbons, 1993). The literature shows that parents change their fertility decisions based on the birth of twins, access to methods of contraceptives and family planning programs.

Since the LLY program induces people to have a smaller family size, the quantity-quality literature will suggest that this will in turn increase parental investments in their daughters. Therefore, it is important to understand how people change their fertility and human capital investment decision in response to deferred financial investments. By including fertility as an outcome, I contribute to the scant literature on the effect of CCT programs on changing parental decisions for number of children and adopting family planning practices.

1.3 Program Description

Madhya Pradesh is one of India's biggest and most populous states with a population of over 75 million people. It is also among the most backward and poor states in the country. Like most socio-economically backward states in India, Madhya Pradesh has historically been a high son preference state and its child sex ratio was 918 females per 1000 males in 2011. [6] There is widespread discrimination against female children in India that has led to a persistent problem referred to as the phenomenon of "missing women" in the literature [Sen, 1989]. One of the reasons for this phenomenon is the non-registration of female births. The share of registered births in total births .The female share in registered births was about 46 percent in Madhya Pradesh before the program. The sex ratio was 841 females per 1000 male registered births.[7] By the year 2015, this has increased to 904 females births per 1000 male births.

There are several programs and policies being launched that are meant to ensure increased survival of female children as well improve their well being. On a similar lines, the Government of Madhya Pradesh announced and implemented Ladli Laxmi Yojana in April, 2007.[8] Under this program, the beneficiaries are first and second born girls born

[6] Source: Census 2011, India. Child sex ratio is defined as number of females per 1000 males in the age group 06 years.

[7] Civil registration System Data, India

[8] $http://ladlilaxmi.com/Sharad_{S}urabhi_{L}adli/Avedan_{T}ype.aspx$

on and after January 1, 2006 in families with income less than the minimum taxable income (annual household income less than Rs.250,000(USD 3800)) and whose parents are residents of the state. Prospective beneficiaries have to register themselves with their village Anganwadi centers. Another major condition is that the parents agree to adopt a terminal method of family planning (female (and (or) male sterilization) after the birth of the second (birth order) daughter and before enrolling her in the program.

As per the program[9], the state government buys National Savings Certificates (NSC)[10] in the joint name of the girl and the designated Project Officer in the block[11]. The amount of NSC bought is Rs. 6000 (USD 100)[12] continuously for the first five years, adding to a total amount of Rs. 30,000 (USD 500). In addition, the girl gets one time cash payment, at different levels of school completion, of Rs. 2000 (USD 33) cash when she gets enrolled in the 6th grade, a sum of Rs. 4000 (USD 67) when she enrolls in the 9th grade and a sum of Rs. 7500 (USD 125) when she enrolls in grade 11. The girls will also receive a monthly allowance of Rs.200 (USD 3) in grades 11 and 12[13]. Once the girl turns 21 she is entitled to get a lump sum amount of Rs. 100,000 (USD 1500) with the maturity of the National Saving Certificates, conditional on her graduating grade 12 and staying unmarried until the age of 18.[14]

The main objectives of the scheme are to improve child sex ratio at birth, increase school enrollment at all three levels - primary, secondary and higher secondary, and discourage child marriage. The program was designed such that the parents get a financial incentive (in the form of a savings bond) as soon as the daughter is born, even though the payout is much farther in the future. Also, the program rewards parents for sending their daughters to school, but the cash payment does not begin until she enrolls in grade

[9] Refer to Table 1

[10] Interest rate 8% per annum

[11] An Indian state is divided into districts and these districts are further divided into blocks.

[12] The exchange rate is $1=60 INR

[13] The benefit is extended only for two years. Thus, in case the girl fails to pass grades 11 and 12 in two years, the benefit would stop by the end of the second year.

[14] The legal age for a woman to get married in India is 18 years.

6. Additionally, there are no performance based conditions, both in terms of schooling and health outcomes, implying that the program is more outcomes based and less action based. Thus, while this program is similar to other CCT programs (there is transfer of payments conditional on school participation), the overall design of the LLY program is quite different. The financial bond bought right at the time of birth helps subsidizing the cost of raising a daughter and attributes to the importance of making sure that parents do not discriminate against their daughters and she is financially empowered when she grows up.

While, the program is well- intentioned, it is not obvious that the program will indeed benefit the girls. Given that there is no immediate payment transfer to the parents, it may not induce credit constrained parents to take the initiative to take better care of and educate their daughters. Parents might not be willing to substitute present consumption in order to get a bigger future payment. In addition, some parents might view the future bond payment as a way to pay dowry in the marriage of their daughter and not necessarily as a way to financially empower their daughter. Therefore, an important research question is: are CCT programs that intervene at the time of birth effective enough to improve the well-being of young girls in a developing country, such as India?

1.4 Data

1.4.1 Registered Births Data

I use the official state panel data from Civil Registration System (CRS) of India, which provides records of registration of births and deaths. I use the annual reports of CRS from 2001-2015 to get total registered births and sex ratio at birth for the registered births. The data for these reports is compiled from the statutory Annual Statistical Report prepared by the Chief Registrars of Births and Deaths of States and Union

Territories. The recorded number of registered births are the births that took place and got registered in the given year itself. That is, for a given year, say 2007, the number of registered births recorded does not include information on births that took place in previous years but got registered late in the year 2007.

1.4.2 Female Share and Fertility Data

In order to assess the individual level outcomes, I use the Indian Human Development Survey 2011-12 (IHDS-II). This is a nationally representative panel data of 42,152 households. The data is available at the individual and household level and is available at the district and state level. It includes household socioeconomic characteristics and a roster of all the members in the household, including ever married women and their fertility history.

This data is ideal for the analyses on sex ratio and changes in fertility, since it was conducted in 2011-12. It has information on all the births, live or still/dead, that took place in a given year. However, given the sampling of the IHDS, this implies that there is a section of women who have not yet completed childbearing. Though the survey asks a question about women's desired number children, using that as a measure of total fertility is potentially endogenous as it is asked at the time of the survey, by when she has already given birth. Moreover, given that I am looking at family size and composition as an outcome, I cannot possibly include family fixed effects.

The first approach to address these issues is to use "non-eligible" women in MP and women in other comparison states. The way I define "eligible" and "ineligible" women is described in detail in the next section. Briefly, a woman who can have a potentially eligible child is defined as treated and the others form the comparison group in MP and non-MP states. I also include state and woman's age fixed effects, along with controls for woman's education and a set of other household level controls. In addition, I conduct a set of robustness checks to control for any differential trends in fertility in MP and comparison states, by including linear and quadratic state-specific women's cohort trends. Second, I drop the youngest (15-19) and the oldest women(41-55) to just look at women who have possibly completed their childbearing. Additionally, the much older women cohort might be very different from the younger women as they would have had children in the distant past under different conditions than the women who became mothers closer to the policy time, so I later drop them from the sample and run the specifications as a robustness check.

I look at sex ratio at birth for all the births (and not just the registered births). Additionally, this data helps enables looking at the birth order of the child, which helps in defining the eligible and non-eligible group of girls. Since the oldest eligible girl (born in 2006) will be 5 or 6 by 2011-12, I look at female share among children at birth.[15] In order to look at how female survival has changed, I look at female share among all the surviving children by the time of the survey, aged 0-12 (birth cohorts 2001-2012). Female share at birth helps us look at the effect on female foeticide whereas female share at older ages helps us look at the effect on female survival and parental neglect of their daughters.

I also use IHDS-II to examine the effects on family sizes, fertility and sterilization outcomes. The data has full birth history of ever married women aged 15-56. This dataset gives us measure of total number of children ever born to a woman and the total number of surviving children by the time of survey. Using this information I can calculate the birth order and sex composition of all the children born to a survey mother.[16] The survey also includes information on methods of contraception (including sterilization) used by a woman (and) or her husband, as well as standard demographic and household characteristics.

[15] using information on all births- children that died after birth and those who were alive by the time of survey

[16] I exclude observations with missing values for year of birth of children. Also, I exclude children who have died. This restriction results in a loss of about 8 percent of the remaining observations.

1.4.3 Schooling and Cognitive Testing Data

I use the Annual Status of Education Report (ASER) 2009-14 to evaluate the educational outcomes. These are nationwide, repeated cross sectional surveys, representative at the state level. The survey covers nearly 700,000 rural children in age group 3-16 per year- those who are currently enrolled, have never been to school, or have dropped out. In each rural district, 30 villages are sampled. In each village, 20 randomly selected households are surveyed. All the children in a selected household, in the age group 5-16 are administered the same tests in basic reading and basic arithmetic.[17]

The survey asks every child aged 5 and above, four questions each in reading and math in their native language. The four reading questions are whether the child can recognize letters; words; read a grade one text and read a grade two text. The reading scores are coded as 1 if the child correctly answers the questions. Then I calculate the *read score* which ranges from 0-4 and is the sum of scores of the four questions.

For testing the math skills, each child is asked to complete four math tasks. The tasks are whether the child can recognize number 1-9; recognize numbers 10-99; subtract and can divide. The math score is coded as 1 if the child correctly completes the tasks. Then I calculate the *math score* which ranges from 0-4 and is the sum of scores of the four tasks for every child.

The typical age at which students enroll in school is 5 or 6. Since the oldest eligible cohort is 8 years old, I use females aged 5-8 to study the schooling and cognitive outcomes.[18] Since the birth cohorts are from 2001-2009 and the exposed birth cohorts are from 2006-2009, this makes it even less plausible for the third and later born children to be in households with eligible girls.

[17] ASER surveyors conduct the surveys on Sundays, when most people are at home and children are not in school, and must return to households if the children are not present at the time of survey.

[18] the latest survey round is 2014, so a girl born in 2006 will be 8 by 2014

1.5 Identification Strategy and Estimating Equations

The objective of my empirical strategy is to assess the causal impact of a conditional cash transfer program in an Indian state on sex ratio, fertility and educational outcomes. I use a difference-in-differences framework to study the effects of the policy on all these outcomes. The incentive structure of the program is given in Table 1. Given this, my regression analyses is based on two main specifications. First, I use difference-in-difference-in-differences to examine the effect of the program on sex ratio at birth and older ages (conditional on survival), and schooling and test scores outcomes of eligible girls. The second specification employs difference-in-differences strategy to estimate the effect of the Ladli Laxmi Yojana on the probability of a marginal birth and the sex of the child; and fertility and sterilization decisions of the parents.

1.5.1 Difference-in-Differences Strategy

Since the program covered first or second born girls born on or after January 1, 2006, I use girls born before 2006 as the comparison group (Duflo, 2001). Using only the first and second born girls before 2006, I employ a double difference frameowrk by using the other five backward states, Chattisgarh, Jharkhand, Uttarakhand, Odisha and Rajasthan as the comparison group. Therefore, I compare outcomes for first and second born girls in MP and comparison states, before and after the program.

Thus, the difference-in-differences equation using the official state panel registration data is:

$$Y_{jc} = \beta_0 + \beta_1 \cdot (MP_j \cdot 2007\&latercohort_c) + \beta_2 \cdot (MP_j \cdot 2006cohort_c) + \delta_j + \gamma_c + U_{jc} \quad (1.1)$$

Y_{jc} is the outcome variable for birth cohort c in state j. Since the policy was announced in 2007 for birth cohorts born from 2006, I add 2007 ($2007\&latercohort$) and 2006 cohorts ($2006cohort$) separately to distinguish the effects on the two birth cohort samples. The policy should not affect the registration of 2006 birth cohorts, as they were already born by the time policy was announced. δ_j and γ_c are the state and cohort fixed effects. β_1 is the double difference estimator that estimates the causal impact of the policy on sex ratio at registered birth and female share in total registered births.

Since, the state panel does not have information on birth order, I cannot use this data to exploit variation in eligibility by birth order. Moreover, the data only has information on registered births but not on the unregistered births. Therefore, I study the effects of the program on sex ratio at birth and later-life survival of females using the individual-level data with information on universe of births: both registered and unregistered. Additionally, I can look at all the births that took place in a given year, irrespective of whether they are alive or dead by the time of survey. Here, I can also use information on birth year and exploit variation in eligibility by using the birth order. The difference-in-differences equation using the individual-level data is:

$$Y_{ijc} = \beta_0 + \beta_1 \cdot (MP_{ij} \cdot After_{ic}) + \delta_j + \gamma_c + \mu X'_{ijc} + U_{ijc} \tag{1.2}$$

Y_{ijc} is the outcome variable for child i from birth cohort c in state j. $After$ is the dummy variable, coded as 1 for birth cohorts 2006 and later. Sample includes only first and second born female children. Specification (2) includes fixed effects for state (δ_j) and birth cohorts (γ_c). X'_{ijc} is a vector of individual and household characteristics controls,which includes mother's age, mother's education, religion, caste, rural and Below Poverty Line (BPL) status. β_1 is the double difference estimator that would estimate the causal impact of the policy on female share at birth and among the older age (0-12) surviving children.

In order to look at fertility and sterilization, I define potentially eligible mothers. A woman who had no or one child by 2005 becomes a potential eligible mother, given that her first or (and) second born child (children) would be eligible if they are girls. Given that these women were making fertility decisions when the program was introduced, I can study how fertility decisions changed because of the program.

I estimate the following difference-in-differences equation for a woman i in state j of age a

$$Y_{ija} = \beta_0 + \beta_1 \cdot (MP_j \cdot Eligiblecase1_{ia}) + \beta_2 \cdot (MP_j \cdot Eligiblecase2_{ia}) + \beta_3 \cdot Eligiblecase1_{ia} +$$

$$+ \beta_4 \cdot Eligiblecase2_{ia} + \omega_a + \delta_j + \mu X'_{ija} + U_{ija} \tag{1.3}$$

where Y_{ija} is the fertility outcome for woman i in state j of age a. $Eligiblecase1$ is a woman who had no child by 2005 and $Eligiblecase2$ is a woman who had just one child by 2005. Specification(3) includes fixed effects for state (δ_j) and womans' age (ω_a). X'_{ijy} is a vector of individual and household characteristics controls,which includes woman's education, religion, caste, rural and Below Poverty Line (BPL) status. The coefficients of interest are β_1 and β_2, to study the effects on two possibly eligible cases of women.

1.5.2 Triple Difference-in-Differences Strategy (DDD)

The main underlying assumption of the DID strategy for the sex ratio and education outcomes, is that in the absence of the program, the outcomes of both girls and women in MP would have followed the same trend as that of the similar children and women in comparison states. I address a majority of omitted variable concerns by comparing the double difference estimates for first and second born females (the treated birth cohorts), with the same estimate for the third and later born females.

A major concern with using third and later born females is if the policy itself changes fertility, using the third and higher birth order children will lead to a selection problem. However, there are two reasons why using the third and later born will not lead to a selection bias in this case. First, the fertility results (which I show in section 6) show that the though the policy does decrease family size, I find that there is no change in people having three or four children. The decline in fertility is mostly for couples who would have had 5 or more children. Second, most of the third born children in the comparison group, given the time period of my analysis (after policy birth cohorts are from 2006-2011), are less likely in the families where the first and second born children would have been eligible. Therefore, these third and late born children are born in the same years as the first and second born children, but will never be eligible for the program. To the extent that there are state specific shifts in investments and other policies or programs for health and education, the higher birth order children will help control for these changes that will affect child health and education. Thus, I use a triple difference framework by exploiting variation by birth order and using the third and later born females as another comparison group, as the preferred specification for sex ratio and education outcomes.

Therefore, the triple-difference estimate of exposure to the CCT program is estimated by:

$$Y_{ijct} = \beta_0 + \beta_1 \cdot (MP_j \cdot After_{ct} \cdot 1st2ndBorn_i) + \beta_2 \cdot (After_c \cdot 1st2ndBorn_i) +$$

$$\beta_3 \cdot (1st2ndBorn_i \cdot MP_j) + \beta_4 \cdot (After_{ic} \cdot MP_j) + \beta_5 \cdot 1st2ndBorn_i$$

$$\delta_j + \gamma_c + \phi_t + \mu X'_{ijct} + U_{ijct} \tag{1.4}$$

where Y_{ijct} is the sex ratio and schooling outcomes for a child i in state j of birth cohort c in year t. $1st2ndBorn$ is a dummy for first and second born child. The sample includes only female children. Specification (4) includes fixed effects for state (δ_j), birth cohorts (γ_c) and year of survey (ϕ_t)(for ASER data). X'_{ijct} is a vector of individual and household characteristics controls. The main parameter of interest is the triple-difference estimate of β_1.

Before I present my results in the next section, a few points must be taken into account. First, inference is based on robust standard errors. Second, I define $After_t$ at the year level as $t \geq 2006$. This is because births that took place during 2006 were conceived before the scheme was announced and are unlikely to have been affected by it. Third, one of the eligibility conditions of LLY is that the family should be non- income tax payers. I do not enforce this condition when defining eligibility in my analyses. This is not a major concern as the number of income-tax payers in India is small due to several tax exemptions.[19] Moreover, there is evidence of widespread tax evasion in India. For these reasons, it is unlikely that the income-tax status of a household is a strictly enforced or a binding condition for eligibility.

1.6 Results

1.6.1 Effects on Sex Ratio

In this section, I test Specification (1) using the official state year data from Civil Registration System and Specifications (2) and (4) using individual-level data. Table 2 presents the descriptive statistics of the main outcomes using the state panel data. Table 3 presents the summary statistics of the main outcomes using the individual data from IHDS-II. Both MP and non-MP comparison states are quite similar before the program is announced. Table 2 shows that for the registered births, the sex ratio and female share in registered births is similar before the program in both MP and comparison states, and is 46% in MP.

Figure 1 plots the cohort-specific effects for female share in total births, for registered births. The figure plots the coefficient of interaction, from equation(1), between the year-of-birth dummy and the treated state (MP). Given that children born in 2006, though

[19]Banerjee and Piketty (2005) show that incomes below the top 1 per cent are largely exempt from taxation in India.

eligible for the program, could not have been the affected cohort when looking at birth registration, I find that relative to the 2005 birth cohort, the 2006 birth cohort seems unaffected. As soon as the program is announced in 2007,the program has a positive effect on sex ratio at birth and female share in total registered births.[20]

The estimation results from Specification (1) for the two outcome variables using the state panel data are reported in Table 5. In columns (1) and (3), the dependent variable is sex-ratio and the female share in total registered births, respectively. In columns (2) and (4), the estimation results are from the weighted least square (WLS) regressions, weighed by the total number of registered births in every year in each state. Using WLS ensures that all the states do not get an equal weight, as the registration can differ by the population of each state.

I find that sex ratio at birth improves in Madhya Pradesh after the implementation of the program and the results are not sensitive to weighing by total registered births in every state. It is important to understand that this analysis does not include the unregistered births and I am able to evaluate the effect on just registered births and find an improvement in sex ratio at birth for registered births. This can be due to an increase in the survival rate of girls at birth or due to an increase in registration of births conditional on survival. Both margins are correlated with better well-being of the girls and is consistent with the program's condition of registering female birth.

In order to make the results comparable with the individual-level data, I estimate the effect of the program on female share in total registered births. In Table 5, columns (3) and (4), I look at the share of female births in total registered births. The results show that even though overall birth registration has gone up in Madhya Pradesh, female share in total registered births is increasing at a faster rate after the implementation of the program in the state.

[20]In Appendix Table A1, I present results for overall effect on registered births by female and male registered births separately respectively and find that both are improving but is higher for female births in the state after the program implementation

Although the state panel data gives us information on just registered births, these results can be discussed in relation to the literature on "missing girls" in India. The literature identifies two types of "missing women", those who are never born through sex-selective abortions and those who go "missing" after birth because of non registration of their births or due to parental neglect leading to child mortality. In this paper, I can look at the changes in the latter using analysis from the state panel data. I find that sex ratio at birth goes up by almost 2 per cent. This implies that gap between global (about 105 males for every 100 females) and Madhya Pradesh's sex ratio at birth (for registered births) closes after the program. This is an important finding since most of the sex ratio gap seems to be because of the unregistered births as shown in Table 3. The channel of non-registration of female births causing missing women seems to be closing after the policy.

There are several benefits of registering the birth of child, like admission to schools, obtaining voter ID, employment in government sector, and marriage registration. Despite these benefits, some parents do not register female births. One reason can be holding themselves un-accountable in case the daughter dies. It can also be due to persistent gender discrimination, parents fail to understand the importance of registering the daughters. Thus, because of the program the number of un-registered female births seems to go down. This is important for the well-being of girls in both short and long run.

A limitation of the official state panel data does not help us distinguish if the increased registration of births implies an increase in female birth registration or an increase in overall sex ratio at birth and a decrease in excess female mortality. The individual-level data includes all births (both registered and not registered) and therefore, gives us a deeper insight on changes in child survival relative to the changes in birth registration conditional on survival. Figure 2 plots the DID coefficients from equation (2) by birth cohorts. This shows the female share for all the registered and un-registered births. I find no change in female share after the policy. Since only the first and second born girls were eligible for the program, I plot the DID coefficients from equation (2) by birth cohorts, and by birth order in Figure 3. Panel A shows the female share among the first and second born children and Panel B shows female share among the third and later born children. Thus, there is no evidence for change in female share among the eligible and non-eligible children, by year of birth and by order of birth.

Given that I do not find any evidence of changes in female share among the third and later born children, this suggests that there is no selection bias when using the gender of the third born child. Thus, using the triple difference framework discussed in equation (4), Table 6 shows the effect of the program on female share at birth and older ages, using the IHDS-II. These results are most comparable to the results in columns (3) and (4) of Table 5. Using the individual-level data, the coefficients for female share are close to zero, but I lose significance at the conventional levels. Therefore, I find no evidence of improvement in female share for the exposed cohort both at birth and at older ages.

Since the results in Table 6 show that there is no evidence of change in sex ratio for the universe of births, it suggests that the estimated increase in registration is mainly because of the program condition of registering births and does not necessarily reflects changes in excess mortality or sex-selective abortions. This shows that parents did not necessarily change their strong son-preference attitudes in favor of having a daughter.

1.6.2 Effects on Fertility

Furthermore, I estimate the effect of Ladli Laxmi Yojana on the total number and composition of children. Since one of the policy conditions for the parents of the eligible girls was to adopt sterilization before enrolling the second born daughter in the program, I expect couples with eligible second born daughters to have smaller family sizes. In fact, if the program strictly enforced the family planning condition of sterilization, then I should find that these families are more likely to stop at two children.

Table 3 shows the descriptive statistics for the non-eligible women in MP and comparison states for the fertility outcomes. On average, a woman is 35 years old and has 3.72 children in MP. Table 7 reports the DID coefficients from specifications (3) using indicators for the likelihood of number of children and the specific number of children as outcomes. Column (1) presents the likelihood of having children and the columns (2)-(6) present the likelihood of having one, two, three, four and five or more children. The sample includes ever-married women with at least one child. All specifications include women's age and state fixed effects. The estimates show that women which started childbearing after 2006 (eligible case 1) are more likely to have two or three children. For women who already had one child by 2005 (eligible case 2), they are less likely to have two children and are more likely to have three or four children. Both types of women are less likely to stop at one child and are less likely to have five or more children. Although I control for a variety of individual and household characteristics, these women may still be different from those observed at much older ages. As a robustness check, I limit my sample to women aged 20-40 and find that the effects remain almost unchanged. These results are reported in Appendix Table A3.

One of the distinctive feature of LLY is that it is more universal in its implementation and both poor and non-poor households are eligible. Therefore, I look at the effect of LLY separately on Below Poverty Line (BPL) and non- BPL households.[21] In Table 7, Panels B and C show the results for poor and non-poor households. I find that BPL families are more likely to have a bigger family size, whereas the non- BPL families seem to show a stronger response to the program conditions of smaller sizes.

In order to test if the effects on the total number of children differ by the gender of the first two children, I estimate specification (3) separately by four different gender combinations of the first two children. These combinations are: boy-girl (BG), girl-girl (GG), Girl-boy (GB), and boy-boy (BB).Given that the program has a condition of

[21]BPL households are identified as those that have a BPL Ration Card.

parents adopting sterilization after the birth of the second daughter, I should find effect on families with specifically the first two composition of children- one with boy-girl composition and those with girl-girl composition of the first two children in that order. Table 8 presents the estimates for this specification. Mothers (eligible case2 women) with first and second born girls (GG) are more likely to have four children. Eligible case 1 women are likely to have three children but I lose significance at the conventional levels. This finding is consistent with the literature suggesting no change in the strong son-preferences among most parents. Mothers with eligible girls (boy-girl and girl- girl combination of first and second born children) are less likely to stop at just two children. Since the government claims all of the eligible families are enrolled in the program and from the findings in this paper, we can conclude that the condition of family planning was not enforced. The next section shows the results for the likelihood of parents adopting sterilization and is another channel through which the program affects family planning decisions.

Effect on Sterilization

One of the conditions of the LLY program was for the couples with a second born daughter to adopt sterilization (either or both the parents). I estimate specification (3) to asses the effect of the program on likelihood of couples getting sterilized. Table 3 presents the descriptive statistics for the non-eligible women in MP and comparison states. Sterilization is a widely prevalent method of family planning in MP as compared to the comparison states. On an average 59 and 49 percent women in MP and comparison states, respectively are sterilized.

To the extent that the program strictly enforced the sterilization condition, I expect that couples with second born daughters are more likely to get sterilized. However, since most parents have a strong son-preference attitude, they might not stop after the birth of daughters. If the program does indeed increase the likelihood of couples to adopt sterilization, I expect to find that the two types of eligible mothers are more likely to get sterilized in MP relative to other states.

Table 9 presents the results for the estimation of specification (3) for the likelihood of couples getting sterilized. I find that there is an overall increase in the likelihood of eligible women getting sterilized in MP relative to other states. Also, eligible case 1 women are more likely to get sterilized after two or three children, whereas eligible case 2 women are more likely to get sterilized after three or four children. These estimates are consistent with the fertility results. Eligible case 1 women are more likely to stop after three children and eligible case 2 women are likely to stop after four children. Thus, even though I do not observe the complete fertility history of these two types of women, I find that they are more likely to have stopped childbearing by the time of survey and adopted sterilization. Additionally, panels B and C of Table 9 show the effects for poor and non-poor households and I find that both households are more likely to respond to the condition of sterilization. However, the response is once again bigger from the non-BPL families.

To examine the effects of the program condition of sterilization after the second daughter, I again assess the likelihood of couple adopting sterilization after the second born daughter. Table 10 presents the results for this estimation, by the gender composition of children. Columns (2) and (3) show estimation for the combinations of children , for which the couple is expected to adopt sterilization under the program. Couples with two girls (girl-girl combination) are less likely to be sterilized. An interesting finding is that parents with boy-girl combination are more likely to adopt sterilization as compared to parents with girl-boy combination of the first two children. This suggests the program had a positive effect on couples adopting sterilization, given that they have an eligible daughter and at least one son. If this was true for couples in general and not necessarily the effect of the program, then I would expect to find the similar effect for couples with girl-boy combination. Coefficients for the girl-boy specification are positive and significant only for case 2 women. Therefore, the results are not necessarily indicative of a general trend in couples adopting sterilization after the birth of son. Additionally, the point estimates for boy-girl are relative bigger than girl-boy combination. This shows that people with an eligible second born daughter did adopt the required sterilization, if the first born is a son. This finding is consistent with the prevalence of strong son preference among Indian parents.

In general, the results for total number of children and likelihood of parents adopting sterilization seems to suggest that parents in MP with eligible daughters did respond to the program. Though the condition of sterilization does not seem to be implemented as was initially designed under the program. Also, the results show that parents are either more likely to have more children or reach their desired family size faster than before the program in MP. In order to understand these channels better, I examine the effect of the program on birth spacing.

Effect on Birth Spacing

This section shows results for birth spacing between the first and second born children for the two types of women after the program. I examine the effect of the program on the birth spacing between the first two births, by the gender of the first born child. Given that the program induces parents to have smaller families with at least one daughter, LLY can possibly increase the probability of sex-selective abortions as parents will most likely want one son and one (eligible) daughter combination among their children. On the other hand, birth spacing can also decrease as parents will want to reach their desired family size faster and gender composition once they have an eligible daughter. This is because, the results in the previous section show that parents are indeed getting sterilized, however not at the margin at which the program intended. Since I find that sex ratio at birth is not changing (when looking at both registered and unregistered births), we know that changes in sex-selective abortions is not the dominant channel here. In order to assess the likelihood of the second, which is a decrease in birth spacing, I estimate specification (3) with birth spacing between the first and second child as an outcome.

Table 11 shows the estimation results for changes in birth spacing as a result of the program. I find that it is indeed the case that couples are more likely to reduce the birth spacing between the first and second child. Additionally, I find that birth spacing remains almost unchanged if the first child is a boy, but decreases if the first child is a girl. These results lend support to the findings from the previous two results on fertility preferences and imply that the eligible mothers are more likely to decrease birth spacing in order to reach their desired family size and composition faster, in order to receive the benefits of the program faster. Overall, I find that mothers who start child bearing after the program are responding differently to the program as compared to those who already had a child by 2005.

1.6.3 Effect on Schooling and Cognition

Table 12 shows the estimates of β_1 from specification(4) using the ASER data for schooling and cognition outcomes. Following the literature, there can be both positive or negative effects on schooling and education outcomes based on the birth order. Therefore, I report results separately for the first and second born daughters. Also, since the results can be confounded by using the higher birth order children as comparison groups, I just use the third born children as a comparison group here. The birth cohorts are from 2001-2009. Given that I just have three after year birth cohorts from 2006-2009, it is unlikely that the third borns are in households with eligible girls. Columns (2) and (3) suggest that LLY had a positive impact on the likelihood of an eligible girl being enrolled in school and on staying enrolled, respectively. Column (1) shows the highest grade completed by the child. This is the highest level of schooling completed by the girl at the time of survey. This is zero if the child was never enrolled in school or if she dropped out in grade 1. For those who are currently enrolled, it is the current grade minus 1 and for those who dropped out, it is the grade in which they dropped out minus one. I find no significant effect of the program on the highest grade completed.

The results in Tables 13 and 14 show the effect of the program on cognition skills of the females. The coefficient for the over all math score is positive for both first and second born daughters but is not significant for the second daughter. The coefficient for reading scores is also positive but I lose significance at the conventional levels.

Therefore, it seems the program had a positive effect on the likelihood of staying enrolled in the school for the eligible girls. However, the effect on cognitive skills is smaller and not significant, but is positive. One reason for these results is that these girls are still quite young and almost 98 percent children enroll in primary school in India. The dropout rates are higher in middle and upper level grades, especially for girls. Therefore, the program is well-intentioned as the one time cash benefits to the girls are given at the time of enrollment in different levels of school completion, where the dropout rates have traditionally been higher. Additionally, it is also possible that not all credit constrained parents respond positively to the program in terms of investing in their daughters education. As the literature shows, parents in poor households associate a high opportunity cost with sending their daughters to school, the results I find are less surprising as I analyze the effects on children in rural households.[22]

1.7 Robustness

My identification strategy relies on the assumption that in the absence of LLY, Madhya Pradesh and the other comparison states did not have systematically different time patterns in the outcome variables. While I show the trends for before years (2001-2005), I can also test whether the results are driven by inclusion of any particular comparison state. Although, all the specifications control for individual and household

[22] ASER survey is done only in rural households

characteristics, I re-estimate specification (1) dropping one comparison state at a time. Table A2 presents these results for the state panel data that are comparable with results in Table 5. The results are still significantly positive, showing increase in number of female registered births.

Since the identification also depends on evaluating just the effects of LLY affecting just the eligible females, it is important to account for simultaneous policies or any other changes that can also potentially affect the treated population. For the fertility results, it is important to account for other government programs that target fertility and family planning decisions of women. Although, I include women's age fixed-effects in all the specifications, I also control for any changes in women's fertility decisions that differ by different age cohorts in different states. However, it is still possible that other programs specific to a comparison states are confounding the results. While I cannot directly control for state-specific time trends because of the single cross-section data, I address this issue by controlling for state-specific women's cohort trends by re-estimating specification (3). If my results are capturing the effect of the LLY program on fertility decisions of parents, I should get robust results by adding this additional control. Tables A5 and A6 report the results and find that fertility and family planning results are robust to adding state-specific women's age cohorts.

Next I conduct a placebo test for the schooling and cognition outcomes, by reassigning the intervention to alternate year. Using the ASER data, I restrict my sample to birth cohorts 1993-2000. Next, I re-assign 1996 as the placebo policy year. This makes children of birth cohorts 1996-2000 as the eligible cohort and 1993-1995 birth cohorts as the comparison group. The comparison states are the same. If my results are capturing the causal impact of LLY on the schooling and cognition outcomes of girls, I should not find significant effects in the placebo regressions. Table A7 shows the results for this placebo test. I do not find significant effects as my main results in Tables 12, 13 and 14. In fact, most of these placebo estimations are close to zero, giving credibility to my estimation strategy and implying a causal interpretation of my main results.

1.8 Conclusion

This paper evaluates the effect of a conditional cash transfer program that differs in features from most conventional CCT programs on sex ratio, birth registration, fertility and parental investment in their daughters.

I find that the incentive of getting a huge financial payment in future is not enough to induce parents to give up the strong son-preference attitude. I find that the program does not have an effect on changing the sex ratio at birth, looking at the universe of births. However, there is a significant increase in the registration of female births. Therefore, there is no effect on female mortality but a positive effect on birth registration. Additionally, the program has a limited yet positive effect on improving the education outcomes of the eligible girls, both in terms of schooling and cognitive skills. With respect to the fertility outcomes, although people with eligible daughters are more likely to be sterilized, parents with two daughters are less likely to stop at two children as well as adopt sterilization. I also find an unintended negative effect of the program, inducing people to have children faster and thereby reducing the birth spacing among children. This is specially worse when the first born is a girl. Oveall however, parents are less likely to have four or more children, but most continue to have at least three children.

There can be several reasons for these effects of the program. First, for the credit constrained households to react positively to the program, the financial benefits are much farther out in the future which can make it difficult for them to re-allocate present day resources. Second, the program is based on outcomes and not actions, like PROGRESA. Parents have to make sure that their daughter graduates from school (grade 12) in order to get the financial payment, therefore, providing them with an incentive to induce improved learning and cognition skills of the daughter. Also given the data, oldest treated cohort is just eight years old and so it may be too early to find comprehensive changes in schooling outcomes for these children.Although, since I already find evidence of positive effect of this program on both schooling and learning outcomes, this seems encouraging and will hopefully grow further at higher grades.[23]

Thus, the analyses presented in this paper have the following policy implications. First, if a less male biased sex ratio is desired, it is desirable to incentivize couples with daughters and not just the first two daughters. This will ensure that parents do not decrease birth spacing when the first born is a daughter, and it will ensure that there are fewer sex selective abortions. Thus, a potential recommendation is to design the program such that parents are incentivized to have daughters, while also allowing them to have a minimum number of desired boys, as an effort to change attitudes towards female discrimination. This will also help ensure less biased attitude towards daughters and will result in fewer "missing women". Second, it is important to understand the difference between credit constrained and non-credit constrained households. In this respect, the policy can be more targeted and incentivize poor households more than the non-poor households.

[23] In India, dropout rates are lower at primary level (about 4%) and much higher at middle and secondary level (grades 6-10), Ministry of Human Resource Development (MHRD) 2014-15

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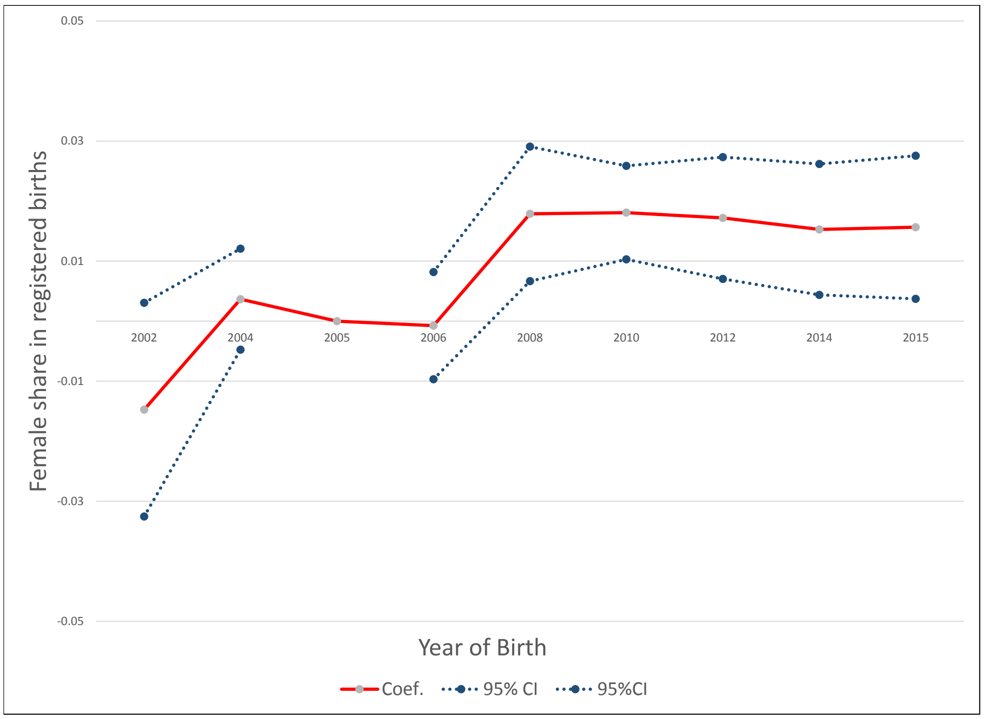
1.9 Figures

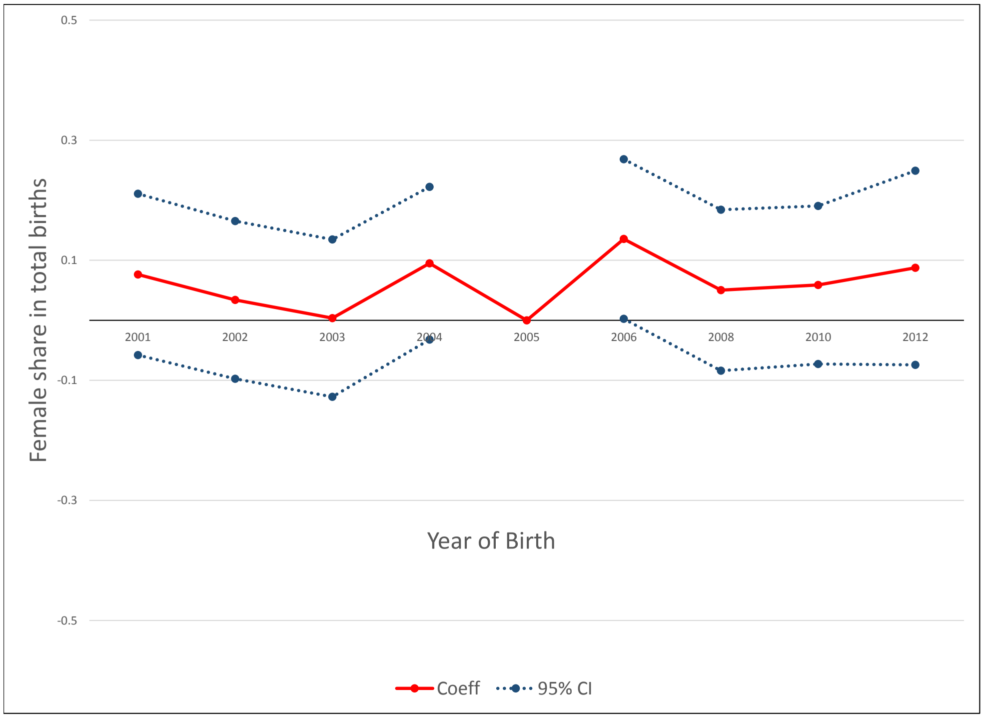
Figure 1.1: **Cohort-specific Effects: Female Share in Registered Births Using Official State Panel Data**

Figure 1.2: Female Share in Registered Births



Notes: Author's calculation of sex ratio and female share at registered births using the Civil Registration System data. This figure plots the difference-in-differences coefficients of the interactions year-of-birth and Madhya Pradesh (MP) dummies from equation (1). Birth cohort 2005 is the omitted reference group from both the specifications.

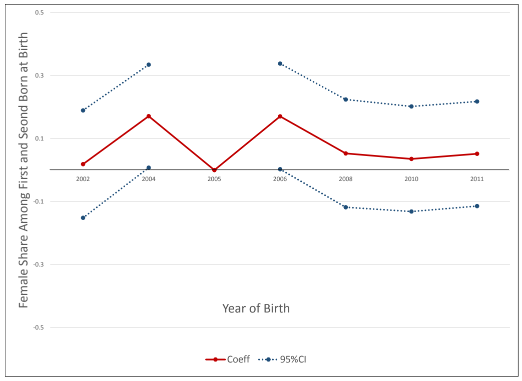
Figure 1.3: **Cohort-specific Effects: Female Share at Birth using Household Survey Data**



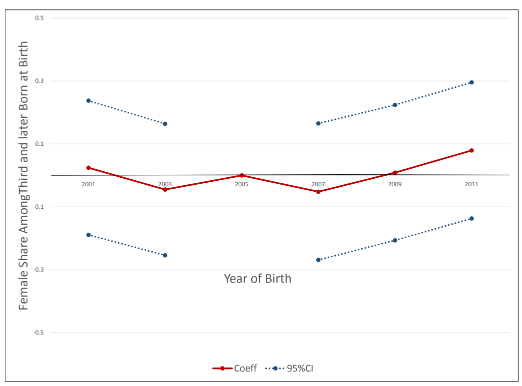
Notes: Author's calculation of female share at birth, for all the births (registered and non-registered births, and for children alive and the ones who died by the time of survey) using the IHDS,2011-12. This figure plots the difference-in-differences coefficients of the interactions year-of-birth and Madhya Pradesh (MP) dummies from equation (2). Birth cohort 2005 is the omitted reference group.

Figure 1.4: **Cohort-specific Effects: Female Share at Birth by Birth Order using Household Survey Data**

(a) First and Second Born Children



(b) Third and Later Born Children



Notes: Author's calculation of female share at birth by birth order, for all the births (those alive by the time of survey) using the IHDS,2011-12. This figure plots the coefficients of the difference-in-differences interactions year-of-birth and Madhya Pradesh (MP) dummies from equation (2). Panel (a) shows the female share among the first and second born children. Panel (b) shows the female share among the third and later born children. Birth cohort 2005 is the omitted reference group from both the specifications.

1.10 Table

Table 1.1: **Incentive Structure of Ladli Laxmi Yojana**

Age/Grade	Amount	Form of Payment
1-5 years	National Saving Certificate of Rs.6000 each for 5 years (Total: Rs.30000) On Maturity: Rs.100,000	Girls can redeem certificates at age 21, **conditional** on: (a) Completing grade 12 (b) Unmarried until 18
Enrolling in Grade 6	Rs.2000	one- time cash
Enrolling in Grade 9	Rs.4000	one-time cash
Enrolling in Grade 11	Rs.7500	one-time cash
During Grades 11-12	Rs.200	monthly

Notes: This table shows the incentive structure of Ladli Laxmi Yojana over time. The state government buys National Savings Certificates for the first five years of the eligible girls' life. These certificates are redeemable for a guaranteed sum of Rs.100,000 (approximately $1600) when the girl turns 21 years of age.

Table 1.2: **Summary Statistics of Key Variables Using Official State Panel Data**

Variables	MP	Non-MP
Sex Ratio	0.85	0.88
	[0.01]	[0.05]
Female Share in total registered births	0.46	0.47
	[0.00]	[0.02]
Log(Female registered births)	12.93	12.26
	[0.23]	[0.74]
Share of total births registered	0.48	0.58
	[0.11]	[0.20]
Observations	6	24

Notes: State-year data on all registered births comes from the Civil Registration System database from year 2001-06. MP is the treated state, Madhya Pradesh. Non-MP states are Chhattisgarh, Jharkhand, Orissa, Rajasthan and Uttaranchal. Sex Ratio is at birth and is calculated as total female births divided by total male births. Female share is measured as the total number of female births registered as the percentage of total registered births. Share of total registered births is defined as the percentage of registered births to the total number of births estimated through Sample Registration System Data.The means of the specified variables are calculated separately for Madhya Pradesh and the control states, for births registered before 2007. Standard deviations appear in brackets.

Table 1.3: **Summary Statistics of Key Variables Using Household Survey Data (Indian Human Development Survey)**

	MP	Non- MP		MP	Non- MP
Sample A: Child-level variables			Sample B: Women-level variables		
Age	2.00	2.03	Age	35.81	36.00
	(1.40)	(1.35)		(5.13)	(5.09)
Schedule Caste	0.18	0.17	Schedule Caste	0.17	0.20
	(0.39)	(0.37)		(0.38)	(0.40)
Schedule Tribe	0.21	0.17	Schedule Tribe	0.20	0.12
	(0.41)	(0.38)		(0.40)	(0.33)
Other Backward Castes	0.42	0.45	Other Backward Castes	0.43	0.42
	(0.49)	(0.50)		(0.49)	(0.49)
Rural	0.82	0.71	Rural	0.81	0.68
	(0.39)	(0.45)		(0.39)	(0.47)
Below Poverty Line	0.37	0.35	Below Poverty Line	0.36	0.32
	(0.48)	(0.48)		(0.48)	(0.47)
Female	0.47	0.49			
	(0.50)	(0.50)			
			Sterilization	0.59	0.49
				(0.49)	(0.49)
			#Children	3.72	3.59
				(1.41)	(1.38)
			Boy-Girl	0.25	0.26
				(0.43)	(0.44)
			Girl-Girl	0.31	0.27
				(0.46)	(0.44)
			Girl-Boy	0.24	0.24
				(0.43)	(0.43)
			Boy-Boy	0.21	0.23
				(0.40)	(0.42)
Observations	1274	1104		2992	5102

Notes: Summary statistics for outcome and control variables in Madhya Pradesh and comparison states (Non-MP). Sample A is the child-level sample of children from birth cohorts 2001-2005. Sample B is the women-level sample of ever married women with at least one child. Eligible case 1 and 2 are women who had no children up to year 2005 and those who had one child by 2005, respectively.The variables are a mean outcome for the women who are in neither of the two categories of eligible women. Below Poverty Line (BPL)are households that have a BPL ration card.

Table 1.4: **Summary Statistics of Key Schooling Variables Using Household Survey Data (Annual Status of Education Report)**

	MP	Non- MP
Panel A: Schooling outcomes		
Highest Grade Completed	1.28	1.20
	(1.21)	(1.22)
Ever Enrolled	0.99	0.98
	(0.10)	(0.15)
Currently Enrolled	0.91	0.88
	(0.28)	(0.33)
Current Grade	2.39	2.36
	(1.20)	(1.21)
Panel B: Math Test Scores		
Math Score	1.38	1.29
	(1.09)	(1.14)
Math Nothing	0.13	0.17
	(0.34)	(0.38)
Numbers 1 to 9	0.36	0.32
	(0.48)	(0.47)
Numbers 10 to99	0.26	0.23
	(0.44)	(0.42)
Subtract	0.11	0.11
	(0.31)	(0.31)
Divide	0.05	0.05
	(0.21)	(0.21)
Panel C: Read Test Scores		
Read Score	1.55	1.44
	(1.23)	(1.29)
Read Nothing	0.13	0.17
	(0.34)	(0.38)
Read Letter	0.33	0.30
	(0.47)	(0.46)
Read Word	0.23	0.20
	(0.42)	(0.40)
Read Grade 1 Text	0.13	0.11
	(0.34)	(0.31)
Read Grade 2 Text	0.09	0.10
	(0.29)	(0.30)
Observations	20760	49284

Notes: Summary statistics for schooling and math test scores in Madhya Pradesh and comparison states (Non-MP). Sample is child-level data of birth cohorts 2001-2009. Highest Grade Completed is the current grade minus 1 for those who are currently attending school and dropout grade minus 1 for those who dropped out. If a child never enrolled then, highest grade is coded as 0. Ever Enrolled is if the child was (is) enrolled in a school. Currently attending school is a dummy if the child is currently enrolled in school. Current grade is the grade in which the child is currently at. Math score ranges from 0-4 and is the sum of scores for the four questions (scored 1 or 0)- nothing; numbers 1 to 9; numbers 10 to99; subtract and divide. Read score also ranges from 0-4 and is the sum of four questions- nothing; letter; word; grade 1 text and grade 2 text. Standard deviation is reported in parentheses.

Table 1.5: **Effect on Female Share and Sex Ratio Using Official State Panel Data from Civil Registration System**

	(1) Sex Ratio	(2) Sex Ratio (Weighted)	(3) Female Share	(4) Female Share (Weighted)
MP*2007&later	0.0716***	0.0663***	0.0200***	0.0185***
	(0.0133)	(0.0141)	(0.00376)	(0.00400)
MP*2006	0.0159	0.0135	0.00443	0.00368
	(0.0137)	(0.0125)	(0.00389)	(0.00352)
Observations	83	83	83	83
Cohort FE	Y	Y	Y	Y
State FE	Y	Y	Y	Y

Notes: Table shows results for equation (1) for outcomes sex ratio at birth and female share at birth for registered births. In columns 2 and 4 estimations are weighted least squares, weighted by total number of registered births in every year in each state. Sample and variables are as described in Table 2. Robust Standard errors are in parentheses. *** p<0.01, ** p<0.05, * p<0.1

Table 1.6: **Effect on Female Share at Birth and at Older Ages using Household Survey Data**

	(1) Female (At Birth)	(2) Female (Ages 0-12)
MP*After* 1st2ndBorn	-0.00828	-0.0173
	(0.0572)	(0.0493)
MP* 1st2ndBorn	-0.0167	-0.00937
	(0.0425)	(0.0308)
After*1st2ndBorn	0.0146	0.0166
	(0.0423)	(0.0348)
MP* After	0.0334	0.0372
	(0.0451)	(0.0395)
Observations	5,405	5,048
Cohort FE	Y	Y
State FE	Y	Y
Year FE	Y	Y
Controls	Y	Y

Notes: Table shows results from equation (2) for outcomes female share at birth and at older ages. Sample includes children aged 0-12 in IHDS 2011-12. For column (1), sample includes all children that are alive or had died by the time of survey. Column (2) includes children that are alive by the time of survey. 1st2nd born is a dummy if the child is either a first or a second born. MP is the state dummy for Madhya Pradesh. After is a birth cohort dummy for children born in 2006 and later. Controls include mother's age and education, caste, religion, BPL and rural. Robust Standard errors are in parentheses. *** $p<0.01$, ** $p<0.05$, * $p<0.1$

Table 1.7: **Effect on Total Number of Children**

	(1)	(2)	(3)	(4)	(5)	(6)
		Number of Children=				
	#child.	1	2	3	4	5 or more
			Panel A: All Women			
MP* Eligible Case 1	0.0729	-0.147***	0.125***	0.0666***	-0.0180	-0.0268***
	(0.0490)	(0.0219)	(0.0276)	(0.0226)	(0.0127)	(0.0103)
MP* Eligible Case 2	0.136**	-0.0469***	-0.0504*	0.0375	0.0865***	-0.0267**
	(0.0600)	(0.0164)	(0.0291)	(0.0287)	(0.0228)	(0.0128)
Observations	11,113	11,113	11,113	11,113	11,113	11,113
			Panel B: BPL Women			
MP* Eligible Case 1	0.353***	-0.169***	0.0578	0.135***	-0.0574**	0.0330*
	(0.0975)	(0.0408)	(0.0520)	(0.0464)	(0.0241)	(0.0197)
MP* Eligible Case 2	0.258**	-0.0229	-0.00132	-0.0785	0.0529	0.0498*
	(0.113)	(0.0312)	(0.0541)	(0.0548)	(0.0411)	(0.0260)
Observations	3,506	3,506	3,506	3,506	3,506	3,506
			Panel C: Non-BPL Women			
MP* Eligible Case 1	0.0688	-0.135***	0.151***	0.0399	-0.00216	-0.0536***
	(0.0577)	(0.0260)	(0.0327)	(0.0253)	(0.0155)	(0.0123)
MP* Eligible Case 2	0.0696	-0.0520***	-0.0789**	0.0946***	0.0950***	-0.0588***
	(0.0716)	(0.0194)	(0.0347)	(0.0338)	(0.0278)	(0.0147)
Observations	7,607	7,607	7,607	7,607	7,607	7,607
Woman's Age FE	Y	Y	Y	Y	Y	Y
State FE	Y	Y	Y	Y	Y	Y
Controls	Y	Y	Y	Y	Y	Y

Notes: This table shows results of having different family sizes for eligible woman. Eligible case 1 is a woman who had no children until 2005. Eligible case 2 is a woman who had just 1 child until 2005. Sample as described in Table 3 (Sample B) and has women who have at least one child. MP is the state dummy for Madhya Pradesh. Controls include controls for woman's education, rural, religion, caste and BPL status. Column (1) shows the likelihood of having children. Columns (2)-(6) show the likelihood of having exactly 1, 2, 3, 4 and 5 or more children, respectively. Panel B and C show results by the BPL status of the household. Robust Standard errors are in parentheses. *** p<0.01, ** p<0.05, * p<0.1

Table 1.8: **Effect on Total Number of Children by Gender of the First Two Children**

	(1)	(2)	(3)	(4)	(5)
		Number of Children=			
	#child.	2	3	4	5 or more
			Panel A: Boy-Girl		
MP* Eligible Case 1	0.109	-0.00518	-0.00689	-0.0582**	0.0703***
	(0.112)	(0.0522)	(0.0567)	(0.0249)	(0.0256)
MP* Eligible Case 2	-0.194	0.0484	-0.00978	-0.0165	-0.0221
	(0.119)	(0.0662)	(0.0656)	(0.0486)	(0.0261)
Observations	2,579	2,579	2,579	2,579	2,579
			Panel B: Girl-Girl		
MP* Eligible Case 1	-0.339***	0.0372	0.0835	-0.00578	-0.115***
	(0.100)	(0.0595)	(0.0659)	(0.0371)	(0.0255)
MP* Eligible Case 2	0.187*	-0.185***	0.0680	0.207***	-0.0900***
	(0.100)	(0.0468)	(0.0605)	(0.0539)	(0.0250)
Observations	2,886	2,886	2,886	2,886	2,886
			Panel C: Girl- Boy		
MP* Eligible Case 1	-0.142	-0.0233	0.0881	-0.00962	-0.0552***
	(0.0863)	(0.0539)	(0.0559)	(0.0259)	(0.0204)
MP* Eligible Case 2	0.104	-0.153***	0.107*	0.0625*	-0.0162
	(0.107)	(0.0573)	(0.0617)	(0.0380)	(0.0299)
Observations	2,562	2,562	2,562	2,562	2,562
			Panel D: Boy-Boy		
MP* Eligible Case 1	0.297***	-0.114**	0.0811	-0.0175	0.0503***
	(0.0879)	(0.0555)	(0.0533)	(0.0303)	(0.0170)
MP* Eligible Case 2	0.304***	-0.0242	-0.0966	0.0442	0.0767***
	(0.112)	(0.0647)	(0.0617)	(0.0415)	(0.0289)
Observations	2,373	2,373	2,373	2,373	2,373
Woman's Age FE	Y	Y	Y	Y	Y
State FE	Y	Y	Y	Y	Y
Controls	Y	Y	Y	Y	Y

Notes: This table shows results of having different family sizes for eligible woman. Eligible case 1 is a woman who had no children until 2005. Eligible case 2 is a woman who had just 1 child until 2005. Sample as described in Table 3 (Sample B) and has women who have at least two children. MP is the state dummy for Madhya Pradesh. Controls include woman's education, rural, religion, caste and BPL status. Column (1) shows the likelihood of having children. Columns (2)-(5) show the likelihood of having exactly 2, 3, 4 and 5 or more children, respectively. Panel A, B, C and D show results by the gender of the first two children in that order. Robust Standard errors are in parentheses. *** p<0.01, ** p<0.05, * p<0.1

Table 1.9: **Effect on Sterilization by the Total Number of Children**

	(1)	(2)	(3)	(4)	(5)
Dep. Var. Sterilized	Full Sample	Has 2 Children	Has 3 Children	Has 4 Children	Has 5 or more Children
		Panel A: All Women			
MP* Eligible Case 1	0.0839***	0.0477	0.205***	0.340**	∅
	(0.0244)	(0.0408)	(0.0582)	(0.155)	
MP* Eligible Case 2	0.0863***	-0.00613	0.113**	0.164**	0.897***
	(0.0296)	(0.0505)	(0.0522)	(0.0715)	(0.0906)
Observations	11,113	3,046	3,342	2,176	1,836
		Panel B: BPL Women			
MP* Eligible Case 1	0.0788*	0.0743	0.328***	∅	∅
	(0.0460)	(0.0836)	(0.0932)		
MP* Eligible Case2	0.0401	-0.127	0.127	0.714***	-0.0242
	(0.0557)	(0.101)	(0.0989)	(0.115)	(0.185)
Observations	3,506	774	1,101	716	748
		Panel C: Non-BPL Women			
MP* Eligible Case 1	0.0927***	0.0264	0.221***	0.426***	∅
	(0.0288)	(0.0477)	(0.0775)	(0.146)	
MP* Eligible Case 2	0.110***	0.0245	0.112*	0.0428	0.912***
	(0.0353)	(0.0592)	(0.0629)	(0.0874)	(0.116)
Observations	7,607	2,272	2,241	1,460	1,088
Woman's Age FE	Y	Y	Y	Y	Y
State FE	Y	Y	Y	Y	Y
Controls	Y	Y	Y	Y	Y

Notes: This table shows results for likelihood of being sterilized (woman or her husband) for the two categories of women, by the total number of children. Eligible case 1 is a woman who had no children until 2005. Eligible case 2 is a woman who had just 1 child until 2005. Sample as described in Table 3 (Sample B) and has women who have at least one child. MP is the state dummy for Madhya Pradesh. Controls include controls for woman's education, rural, religion, caste and BPL status. Column (1) shows the likelihood of adopting sterilization for the entire sample. Columns (2)-(5) show the likelihood of adopting sterilization if the woman has exactly 2, 3, 4 and 5 or more children, respectively. Panel B and C show results by the BPL status of the household. ∅ Sample does not have any eligible case 1 woman with 5 or more children in MP and none for BPL households with 4 or more children. Robust Standard errors are in parentheses. *** p<0.01, ** p<0.05, * p<0.1

Table 1.10: **Likelihood of being sterilized after the policy, by gender of the first two children**

Dep. Var. Sterilized	All	BG	GG	GB	BB
		Panel A: All Women			
MP* Eligible Case 1	0.0839***	0.158**	-0.170***	0.0508	0.374***
	(0.0244)	(0.0623)	(0.0350)	(0.0577)	(0.0569)
MP* Eligible Case 2	0.0863***	0.219***	0.0518	0.118*	0.0831
	(0.0296)	(0.0676)	(0.0517)	(0.0624)	(0.0667)
Observations	11,113	2,579	2,886	2,562	2,373
		Panel B: BPL Women			
MP* Eligible Case 1	0.0788*	0.136	-0.212***	-0.0197	0.532***
	(0.0460)	(0.141)	(0.0677)	(0.109)	(0.105)
MP* Eligible Case2	0.0401	0.255**	-0.00769	0.0189	0.0847
	(0.0557)	(0.121)	(0.105)	(0.122)	(0.112)
Observations	3,506	805	907	824	803
		Panel C: Non- BPL Women			
MP* Eligible Case 1	0.0927***	0.160**	-0.193***	0.0597	0.327***
	(0.0288)	(0.0721)	(0.0344)	(0.0707)	(0.0687)
MP* Eligible Case2	0.110***	0.162**	0.100*	0.161**	0.0633
	(0.0353)	(0.0818)	(0.0599)	(0.0726)	(0.0840)
Observations	7,607	1,774	1,979	1,738	1,570
Woman's Age FE	Y	Y	Y	Y	Y
State FE	Y	Y	Y	Y	Y
Controls	Y	Y	Y	Y	Y

Notes: This table shows results for likelihood of being sterilized (woman or her husband) for the two categories of women, by the gender of the first two children in that order. Eligible case 1 is a woman who had no children until 2005. Eligible case 2 is a woman who had just 1 child until 2005. Sample as described in Table 3 (Sample B) and has women who have at least two children. MP is the state dummy for Madhya Pradesh. Controls include controls for woman's education, rural, religion, caste and BPL status. Panel B and C show results by the BPL status of the household. Robust Standard errors are in parentheses. *** p<0.01, ** p<0.05, * p<0.1
BG= Boy-Girl; GG= Girl-Girl; GB= Girl-Boy; BB- Boy- Boy

Table 1.11: **Effect on Birth Spacing between the first and second child**

	(1)	(2)	(3)
Dep.Var. Birth spacing	All	First Born is Boy	First Born is Girl
	Panel A: All Women		
MP* Eligible Case 1	-0.0358	0.00863	-0.106
	(0.0731)	(0.107)	(0.100)
MP* Eligible Case2	-0.173	0.210	-0.512***
	(0.130)	(0.221)	(0.146)
Observations	10,400	4,952	5,448
	Panel B: BPL		
MP* Eligible Case 1	-0.128	0.302	-0.370*
	(0.158)	(0.238)	(0.204)
MP* Eligible Case2	-0.173	0.328	-0.733**
	(0.232)	(0.337)	(0.310)
Observations	3,339	1,608	1,731
	Panel C: Non-BPL		
MP* Eligible Case 1	0.0297	-0.0761	0.110
	(0.0842)	(0.120)	(0.122)
MP* Eligible Case2	-0.124	0.233	-0.372**
	(0.156)	(0.282)	(0.169)
Observations	7,061	3,344	3,717
Woman's Age FE	Y	Y	Y
State FE	Y	Y	Y
Controls	Y	Y	Y

Notes: This table shows results for birth spacing between the first and second born child, conditional on having at least two children. Eligible case 1 is a woman who had no children until 2005. Eligible case 2 is a woman who had just 1 child until 2005. Sample as described in Table 3 (Sample B) and has women who have at least two children. MP is the state dummy for Madhya Pradesh. Controls include controls for woman's education, rural, religion, caste and BPL status. Panel B and C show results by the BPL status of the household. Robust Standard errors are in parentheses. *** p<0.01, ** p<0.05, * p<0.1

Table 1.12: **Effect on Schooling Outcomes for Females Aged 5-8 Years**

	(1) Highest Grade Completed	(2) Ever Enrolled	(3) Currently Attending School	(4) Current Grade
MP* After * 1stBorn	0.00724	0.00862**	0.0261**	-0.00703
	(0.0333)	(0.00407)	(0.0119)	(0.0387)
MP* After * 2ndBorn	0.00688	2.15e-05	0.0250**	-0.00698
	(0.0306)	(0.00397)	(0.0112)	(0.0361)
MP* 1stBorn	-0.0189	-0.00904***	-0.0141**	-0.00511
	(0.0232)	(0.00261)	(0.00605)	(0.0245)
After * 1stBorn	-0.0307*	-0.000447	-0.0257***	-0.0412**
	(0.0176)	(0.00261)	(0.00676)	(0.0207)
MP* 2ndBorn	-0.0206	-0.00317	-0.00366	-0.0192
	(0.0185)	(0.00219)	(0.00500)	(0.0198)
After * 2ndBorn	-0.0201	0.00257	-0.0101	-0.0238
	(0.0166)	(0.00252)	(0.00642)	(0.0200)
MP* After	0.00199	-0.0126***	-0.0234***	0.0485*
	(0.0227)	(0.00310)	(0.00855)	(0.0273)
Observations	112,073	112,073	112,073	93,639
Year of Birth FEs	Y	Y	Y	Y
Year FEs	Y	Y	Y	Y
State FE	Y	Y	Y	Y
Controls	Y	Y	Y	Y

Source: ASER 2009-2014

Notes: The table shows result for schooling outcomes for females aged 5-8 years. Sample as described in Table 4. Controls include control for mother's age. Robust Standard errors are in parentheses. *** p<0.01, ** p<0.05, * p<0.1

Table 1.13: **Effect on Math Scores of Females Aged 5-8 Years**

	(1) Math Score	(2) Nothing	(3) Numbers 1 to 9	(4) Numbers 10 to 99	(5) Subtract	(6) Divide
MP* After* 1st born	0.0568*	0.00402	-0.0188	-0.0276**	0.0230***	0.0155***
	(0.0314)	(0.0146)	(0.0163)	(0.0130)	(0.00806)	(0.00539)
MP* After*2ndBorn	0.0229	-0.0117	-0.00305	0.00355	0.00820	-0.00144
	(0.0279)	(0.0137)	(0.0147)	(0.0112)	(0.00680)	(0.00457)
MP*1st born	-0.172***	0.0350***	0.0186*	0.00848	-0.0396***	-0.0222***
	(0.0232)	(0.00797)	(0.0110)	(0.0100)	(0.00698)	(0.00473)
After* 1st born	0.0166	0.00142	0.0283***	0.0266***	-0.00633	-0.0115***
	(0.0184)	(0.00784)	(0.00867)	(0.00706)	(0.00486)	(0.00322)
MP*2ndBorn	-0.0569***	0.0200***	-0.00418	-0.00174	-0.0117**	-0.00355
	(0.0187)	(0.00652)	(0.00884)	(0.00789)	(0.00563)	(0.00384)
After*2ndBorn	0.0263	0.00603	0.00633	0.0164***	-0.00114	-0.00236
	(0.0163)	(0.00746)	(0.00794)	(0.00617)	(0.00404)	(0.00264)
MP* After	-0.222***	0.0889***	-0.0183*	-0.0438***	-0.0271***	-0.00865***
	(0.0200)	(0.0103)	(0.0108)	(0.00790)	(0.00472)	(0.00319)
Observations	112,073	112,073	112,073	112,073	112,073	112,073
Year of Birth FEs	Y	Y	Y	Y	Y	Y
Year FEs	Y	Y	Y	Y	Y	Y
State FE	Y	Y	Y	Y	Y	Y
Controls	Y	Y	Y	Y	Y	Y

Source: ASER 2009-2014
Notes: The table shows estimates for math skills of females aged 5-8 years. Sample as described in Table 4. Controls include control for mother's age. Robust Standard errors are in parentheses. *** p<0.01, ** p<0.05, * p<0.1

Table 1.14: **Effect on Read Scores of Females Aged 5-8 Years**

	(1) Read Score	(2) Nothing	(3) Letter	(4) Word	(5) Grade I Text	(6) Grade II Text
MP* After* 1st born	0.0187	-0.00730	-0.00385	-0.0184	0.0165*	0.00245
	(0.0375)	(0.0147)	(0.0159)	(0.0119)	(0.00887)	(0.00873)
MP* After* 2ndBorn	0.0126	-0.0223	0.00922	0.00768	0.00384	-0.00587
	(0.0331)	(0.0138)	(0.0143)	(0.0104)	(0.00764)	(0.00725)
MP* 1st born	-0.185***	0.0415***	0.0146	0.00682	-0.0244***	-0.0350***
	(0.0271)	(0.00794)	(0.0107)	(0.00952)	(0.00738)	(0.00701)
After* 1st born	0.0326	0.00443	0.0254***	0.0158**	0.00627	-0.0108**
	(0.0218)	(0.00794)	(0.00840)	(0.00644)	(0.00494)	(0.00519)
MP* 2ndBorn	-0.0633***	0.0182***	-0.00186	0.000935	-0.00742	-0.0103*
	(0.0212)	(0.00647)	(0.00861)	(0.00766)	(0.00606)	(0.00526)
After* 2ndBorn	0.0287	0.0121	0.00527	0.00739	-0.000852	0.00281
	(0.0193)	(0.00760)	(0.00766)	(0.00569)	(0.00428)	(0.00428)
MP* After	-0.261***	0.0936***	-0.000708	-0.0456***	-0.0388***	-0.0131***
	(0.0237)	(0.0104)	(0.0105)	(0.00746)	(0.00541)	(0.00500)
Observations	112,073	112,073	112,073	112,073	112,073	112,073
Year of Birth FEs	Y	Y	Y	Y	Y	Y
Year FEs	Y	Y	Y	Y	Y	Y
State FE	Y	Y	Y	Y	Y	Y
Controls	Y	Y	Y	Y	Y	Y

Source: ASER 2009-2014
Notes: The table shows estimates for reading skills of females aged 5-8 years. Sample as described in Table 4. Controls include control for mother's age. Robust Standard errors are in parentheses. *** p<0.01, ** p<0.05, * p<0.1

1.11 Appendix

Table A1: **Impact of the Program on Birth Registration Using State Panel Data**

	(1) log (female regd.)	(2) log (male regd.) (Weighted)	(3) log(female regd.)	(4) log(male regd.) (Weighted)	(5) Regd. share
MP* 2007&later	0.269***	0.192**	0.225***	0.154**	0.143***
	(0.0979)	(0.0896)	(0.0769)	(0.0694)	(0.0509)
Observations	83	83	83	83	89
Cohort FE	Y	Y	Y	Y	Y
State FE	Y	Y	Y	Y	Y

Notes: Sample is the same as described in Table 2. Log (female regd.) and log (male regd.) are the log values of total number of female and male births registered, respectively. Columns (3) and (4) are weighted least square estimations of columns (1) and (2), weighted by the total number of registered births in every year, in each state. Regd. share is the share of registration as a percentage of total estimated number of births. Robust Standard errors are in parentheses. *** p<0.01, ** p<0.05, * p<0.1

Table A2: **Robustness Check by Dropping Comparison States One by One**

	(1)	(2)	(3)	(4)	(5)
	State Dropped				
	Chhatisgarh	Jharkhand	Odisha	Rajasthan	Uttarakhand
	Coefficient of MP *2007&later				
Sex Ratio	0.0675***	0.0745***	0.0547***	0.0767***	0.0683***
	(0.0139)	(0.0112)	(0.0101)	(0.0128)	(0.0123)
Sex Ratio(Weighted)	0.0629***	0.0661***	0.0468***	0.0819***	0.0631***
	(0.0137)	(0.0125)	(0.0110)	(0.0118)	(0.0126)
Observations	68	72	68	68	71
Female Share	0.0189***	0.0209***	0.0154***	0.0215***	0.0191***
	(0.00390)	(0.00316)	(0.00290)	(0.00357)	(0.00348)
Female Share(Weighted)	0.0176***	0.0185***	0.0132***	0.0228***	0.0177***
	(0.00389)	(0.00357)	(0.00321)	(0.00328)	(0.00359)
Observations	68	72	68	68	71
Log(Female regd births)	0.220**	0.335***	0.163	0.346***	0.266**
	(0.0967)	(0.0868)	(0.0991)	(0.119)	(0.103)
Log(Female regd births)(Weighted)	0.214***	0.259***	0.0930	0.356***	0.225***
	(0.0771)	(0.0727)	(0.0646)	(0.102)	(0.0787)
Observations	68	72	68	68	71
Share of Registration	0.116**	0.186***	0.117**	0.187***	0.108**
	(0.0486)	(0.0534)	(0.0574)	(0.0581)	(0.0483)
Share of Registration(Weighted)	0.0661*	0.0968**	0.0250	0.164***	0.0798**
	(0.0354)	(0.0381)	(0.0358)	(0.0440)	(0.0384)
Observations	72	72	72	72	75
State FEs	Y	Y	Y	Y	Y
Cohort FEs	Y	Y	Y	Y	Y

Notes: Sample as described in Table 2. This table reports the DID coefficients from equation(1) when comparison states are dropped one at a time. Outcomes with (Weighted) are weighted least square estimations of the equation (1), weighted by the total number of registered birth in every year, in each state, excluding the state that is dropped in the respective specifications. Robust Standard errors are in parentheses. *** p<0.01, ** p<0.05, * p<0.1

Table A3: **Effect on Total Number of Children, Women Aged 20-40**

	(1)	(2)	(3)	(4)	(5)	(6)
		Number of Children				
	#child.	1	2	3	4	5 or more
MP*Eligible Case1	0.156***	-0.150***	0.119***	0.0726***	-0.0339**	-0.00749
	(0.0498)	(0.0221)	(0.0283)	(0.0234)	(0.0136)	(0.0107)
MP*Eligible Case2	0.210***	-0.0402**	-0.0646**	0.0391	0.0729***	-0.00709
	(0.0606)	(0.0158)	(0.0296)	(0.0295)	(0.0236)	(0.0131)
Observations	9,713	9,713	9,713	9,713	9,713	9,713
Women's Age FE	Y	Y	Y	Y	Y	Y
State FE	Y	Y	Y	Y	Y	Y
Controls	Y	Y	Y	Y	Y	Y

Notes: This table shows results for total number of children, for women aged 20-40. Sample as described in table 3 and has women with at least one child. All specifications have women's age fixed effects and state fixed effects and controls for individual and household characteristics. Robust Standard errors are in parentheses. *** p<0.01, ** p<0.05, * p<0.1

Table A4: **Effect on Sterilization by Number of Children, women aged 20-40**

	(1)	(2)	(3)	(4)	(5)
		Number of Children			
	#child.	2	3	4	5 or more
MP*Eligible Case1	0.0854***	0.0304	0.206***	0.312**	
	(0.0252)	(0.0420)	(0.0590)	(0.156)	
MP*Eligible Case2	0.0870***	-0.0339	0.109**	0.174**	0.850***
	(0.0304)	(0.0515)	(0.0531)	(0.0722)	(0.0875)
Observations	9,713	2,766	2,962	1,888	1,460
Women's Age FE	Y	Y	Y	Y	Y
State FE	Y	Y	Y	Y	Y
Controls	Y	Y	Y	Y	Y

Notes: This table shows results for likelihood of getting sterilized, for women aged 20-40. Sample as described in Table 3 has women with at least one child. All specifications have women's age fixed effects and state fixed effects. Robust Standard errors are in parentheses. *** p<0.01, ** p<0.05, * p<0.1

Table A5: **Effect on Total Number of Children with state specific women's cohort trends**

	(1)	(2)	(3)	(4)	(5)	(6)
	# child.	# child.=1	# child.=2	# child.=3	# child.=4	# child.¿=5
MP*Eligible Case1	0.207***	-0.161***	0.113***	0.0823***	-0.0425**	0.00810
	(0.0747)	(0.0241)	(0.0334)	(0.0305)	(0.0213)	(0.0182)
MP*Eligible Case2	0.205***	-0.0558***	-0.0559*	0.0487	0.0718***	-0.00885
	(0.0714)	(0.0200)	(0.0316)	(0.0308)	(0.0242)	(0.0156)
Observations	11,113	11,113	11,113	11,113	11,113	11,113
StatexWoman's Cohort Trend	Y	Y	Y	Y	Y	Y
Women's Age FE	Y	Y	Y	Y	Y	Y
State FE	Y	Y	Y	Y	Y	Y
Controls	Y	Y	Y	Y	Y	Y

Notes: This table shows results for total number of children. Sample as described in Table 3 and has women with at least one child. All specifications include state-specific women's cohort trends, and women's age and state fixed effects. Controls added for individual and household characteristics. Robust Standard errors are in parentheses. *** p<0.01, ** p<0.05, * p<0.1

Table A6: **Effect on Sterilization by Number of Children, with state specific women's cohort trends**

	(1) sterilized # child.	(2) sterilized # child.=2	(3) sterilized # child.=3	(4) sterilized # child.=4	(5) sterilized # child.¿=5
MP*Eligible Case1	0.0815**	0.00424	0.166**	0.333**	
	(0.0322)	(0.0559)	(0.0688)	(0.159)	
MP*Eligible Case2	0.0832***	-0.0380	0.0853	0.188**	0.729***
	(0.0321)	(0.0552)	(0.0594)	(0.0834)	(0.115)
Observations	11,113	3,046	3,342	2,176	1,836
StatexWoman's Cohort Trend	Y	Y	Y	Y	Y
Women's Age FE	Y	Y	Y	Y	Y
State FE	Y	Y	Y	Y	Y
Controls	Y	Y	Y	Y	Y

Notes: This table shows results for likelihood of getting sterilized (woman or her husband). Sample as described in Table 3 and has women with at least one child. All specifications include state-specific women's cohort trends, and women's age and state fixed effects. Controls added for individual and household characteristics. Robust Standard errors are in parentheses. *** p<0.01, ** p<0.05, * p<0.1

Table A7: **Placebo Test: Reassign intervention to alternate age groups**

	(1) Ever Enrolled	(2) Currently Enrolled	(3) Current Grade	(4) Highest Grade Completed	(5) Math Score	(6) Read Score
MP* Psuedoafter* firstborn	-0.00304	0.0216	-0.224	-0.0404	-0.0817	-0.115
	(0.0154)	(0.0314)	(0.161)	(0.199)	(0.0920)	(0.124)
MP* Psuedoafter*secondborn	0.000257	0.0460	-0.192	0.0261	-0.0379	-0.0960
	(0.0162)	(0.0328)	(0.167)	(0.207)	(0.0956)	(0.129)
Psuedoafter*firstborn	0.00605	-0.0269	-0.0369	-0.0786	0.114**	0.188**
	(0.0113)	(0.0207)	(0.0964)	(0.131)	(0.0564)	(0.0743)
MP* Psuedoafter	-0.0171	-0.0785**	0.399**	-0.0823	-0.164*	-0.222*
	(0.0151)	(0.0308)	(0.158)	(0.195)	(0.0907)	(0.122)
Psuedoafter*secondborn	0.00635	-0.0194	0.0469	0.00902	0.0743	0.154**
	(0.0118)	(0.0214)	(0.100)	(0.136)	(0.0584)	(0.0770)
MP* firstborn	-0.000593	-0.0378	0.117	-0.0143	0.00479	0.0129
	(0.0153)	(0.0313)	(0.160)	(0.198)	(0.0911)	(0.123)
MP* secondborn	-0.00270	-0.0476	0.173	-0.0236	0.0114	0.0541
	(0.0161)	(0.0327)	(0.166)	(0.207)	(0.0947)	(0.127)
Observations	267,465	267,465	242,749	264,535	267,465	267,465

Notes: This table shows the results for the schooling and math and reading scores for females of birth cohorts 1993-2000, using specification (4). In this placebo test, I re-assign the policy to an alternate year, such that birth cohorts from 1996-2000 become the affected cohort. Psuedoafter is the policy year 1996. Robust standard errors in parentheses. *** $p<0.01$, ** $p<0.05$, * $p<0.1$

Chapter 2

Spillover Effects of a Conditional Cash Transfer Program on Ineligibles: Evidence from India

2.1 Introduction

Policy interventions such as the conditional cash transfers (CCT) are likely to play an important role in affecting the human capital development of all individuals in a family with a treated child. The broad literature on program evaluation focuses mainly on the own effects and not so much on the spillover effects on the ineligible siblings in a treated family. In particular, little is known about the spillover effects of these programs on the siblings in the same households, and their effect on decisons of intra-household resource allocation. This paper estimates these indirect effects of the conditional cash transfer program in India, Ladli Laxmi Yojana (LLY), on the education outcomes of ineligible siblings, and studies the possible mechanisms through which these spillover effects occur.

The government of an Indian state, Madhya Pradesh (MP), introduced Ladli Laxmi Yojana program on similar lines aimed at improving the education and health outcomes of girls through financial transfers. The program was announced in 2007 and is the flagship conditional cash transfer program in the state. The program eligibility begins right from the time of birth but it only covers the first or second born girls in a family. The payments made are deferred until the girls enroll in grade 6 and are later made upon meeting the different schooling levels. The biggest payment, however, comes at age 21 and is given directly to the girl. This large payout is about twice the size of per capita income of the state. This financial incentive is in the form of a longterm savings bond redeemable on the daughters 21st birthday conditional on her being unmarried until the age of 18 and completing school level education. This raises the issue of how liquidity constrained households may respond to the program by possibly reallocating the resources away from the ineligible sibling towards the eligible sibling. If this is the case, the program will have negative effect on the human capital development of the ineligible siblings. Another possibility can be that parents become more inclined to educate all their children and the program has positive spillover effects on siblings.

Understanding the spillover effects of such a program and the plausible mechanisms for these effects is important because of several reasons. First, the study of spillover effects has implications for the design of such policies and programs. Second, this type of program is now being implemented in other similar Indian states and therefore it is important to evaluate both the own and indirect effects in terms of spillovers. Finally and more broadly, this enables us to understand the preferences and changes in household decision making that occur because of these programs.

This paper is the first to provide evidence on the spillover effects of Ladli Laxmi Yojana in India. I assess the impact of LLY on the education outcomes, particularly schooling outcomes and test scores, of older ineligible siblings of the treated girls. A priori, the effect on the education outcomes of the ineligible children in the treated families is unclear. For example, after the program, parents may be encouraged to invest more in the overall human capital development of all of their children, which will lead to positive spillover effects of the program on all the children in a treated family. On the contrary, given that the cash transfers and the big payout are conditional on the girl completing different levels of schooling and eventually graduating from high school by age 21, parents might divert resources away from the ineligible children towards the eligible children. This will result in negative effects on the schooling outcomes of the ineligible older siblings in the treated families. These two opposing stories imply that the effect of such a program on the other ineligible children in treated families is an open question.

The spillover effects of a conditional cash transfer program are not that widely discussed in the literature, and show both positive and negative spillover effects (Barrera-Osorio et al., 2008; de Janvry and Sadoulet, 2006; Ferreira et al.,2009). To date, there are only a few studies that look at the effects of CCTs in the Indian context (Anukriti, 2017; Sinha and Yoong, 2009). Most of these look at the effects of these programs on the treated and not on the ineligibles. This study adds to the under-studied literature on this topic by using a distinctive program: one that allows for variation in exposure by state, birth cohort and birth order.

Using an individual level schooling and test scores data from a nationally representative household survey, Annual Status of Education Report (ASER), I exploit variation in exposure across birth years, states, and birth order to estimate the effects of LLY on the ineligible older siblings in a household. Employing a difference-in-differences framework, I compare outcomes of ineligible older siblings of treated girls to older siblings of non-treated girls in Madhya Pradesh and comparison states. I find that Ladli Laxmi Yojana had no effect on the schooling outcomes of the older ineligible siblings. However, there is some evidence of parents decreasing investments in the education of ineligible siblings as I find evidence of reduced math and reading skills.

Using exogenous variation in the eligibility of the subset of eligible children in a household, I can identify spillover effects in the schooling decisions among children who were ineligible older siblings of the program-eligible girls. The use of this natural experimental design enables me to overcome the many challenges of identification problems for this program's impact. I define treatment in the following way, I treat children with younger program-eligible sibling (or the second born sister) as treated and the ones who do not have any eligible sibling as the comparison group in Madhya Pradesh as the treated group. Then use the other five states, Chattisgarh, Jharkhand, Orissa, Rajasthan and Uttarakhand as comparison states.

The key dependent variables of interest are school enrollment, probability of staying enrolled in school, grade progression, and math and reading test scores. Since, the effects can also differ by the gender of the older sibling, I evaluate the results separately for the two groups. My main findings are as follows: First, I find no evidence of a change in the schooling outcomes of either the older ineligible sister or the brother. However,I find a negative effect on math and reading skills of the ineligible older siblings. One possible concern with the identification strategy is that there are other potential mechanisms through which the ineligible siblings may have been affected. The focus of LLY was to educate girls and improve their wellbeing by providing cash payments at different levels of school completion. This would have induced behavioral changes among the households with eligible girls along several dimensions, leading to the concern that the spillovers can be due to peer effects or can be a behavioral response among the households with program-eligible girls.

There are two possible explanations for the results I find. First, although I do not find any evidence of change in school quality because of the program, I cannot completely rule out the hypothesis that LLY did not adversely affect that, as schools could have become more crowded impacting the teacher quality or effort indirectly. Second alternative interpretation for my findings is that the households with no eligible girls may have responded positively to the information provided regarding the benefits of attaining education. Additionally, if the program-eligible girls are in credit constrained households then the parents will most likely reallocate their resources away from ineligible siblings towards eligible girls. This will lead to an overall negative effect on the schooling outcomes of the ineligible siblings of the eligible girls.

There are, however, several other hypotheses that can be rejected. I do not find any evidence that the program affected eligible children's health, which may have led to better schooling outcomes. In a parallel study, (Jain, 2018) evaluating the own effects of the program, I find that the positive effect on schooling outcomes of the eligible girls are not driven by an improvement in their health. Additionally, there can be concerns about differences in state specific trends in schooling outcomes, and therefore the results I find are not the effect of the exposure to the program. Although, I have state and cohort birth fixed effects, I add control for year-of-birth of the second born sister interacted with that of the treated state.

The rest of this paper is as follows. Section 2 describes the program; section 3 describes the data, and discusses the estimating equations, and identification strategy; section 4 presents the main results; section 5 shows robustness checks and section 6 concludes.

2.2 Program Description

The government of Madhya Pradesh, one of India's biggest, poorest, and most populous states, introduced a conditional cash transfer program, Ladli Laxmi Yojana. The program was introduced with the intent to induce parents to invest in human capital development of their daughters and promote well-being of girls. The program has various distinct characteristics that make it different in design from other CCT programs. There are three main dimensions of variability in program eligibility that help identify the effect of this program on the ineligible children in families with an eligible child. The beneficiaries of the program are first and second born girls born on and after January 1, 2006 in MP and in families with income less than the minimum taxable income (annual household income less than Rs.250,000(USD 3800)). Therefore, the program has variability with respect to gender, birth order and state. Another condition under the program is that the parents agree to adopt a terminal method of family planning (vasectomy or mastectomy) after the birth of second (birth order)daughter and before enrolling her in the program. However, in a parallel study, (Jain, 2018) I find that this condition was not implemented as design and most families continue to have three or more children.

Under the program, the state government buys National Savings Certificates of Rs.6000 each for the first five years of the life of the girl. These certificates are redeemable when the girl turn 21 for Rs.100,000. This is the biggest payment made under the program is almost the double the amount of the per capita income of the state. However, the certificates are redeemable on meeting two conditions, the girl stays unmarried until the legal age of 18 and completes schooling until grade 12. In order to further induce schooling, the girl also receives a cash payment on enrolling at different levels of schooling. She receives as sum of Rs. 2000 (USD 33) cash when she gets enrolled in the 6th grade, a sum of Rs. 4000 (USD 67) when she enrolls in the 9th grade and a sum of Rs. 7500 (USD 125) when she enrolls in grade 11. The girls will also receive a monthly allowance of Rs.200 (USD 3) in grades 11 and 12.

The main objectives of the scheme is to improve of the welfare of the girls, to increase school enrollment, and discourage child marriage by subsidizing the cost of having a daughter. While the program endows benefits for the eligible girl in the family, only the first and second born girls are eligible under the program. Therefore, it is not obvious that the program will benefit all the girls in a family. In terms of spillover effects, since every child is not eligible under the program, parents may get inclined to reallocate resources away from the ineligible children towards the eligible children. This may especially be true in a credit constrained family, as in order to get the monetary benefits parents may be constrained to invest in just the eligible girl. Therefore, an important research question is: what are the spillover effects of a CCT programs with such an eligibility design?

2.3 Data and Identification Strategy

2.3.1 Data

I use the individual-level Annual Status of Education Report (ASER) 2009-14 to evaluate the educational outcomes. The study uses all the five rounds of cross-sectional data from these surveys that are nationwide, repeated cross sectional surveys, representative at the state level. The survey covers nearly 700,000 rural children in age group 3-16 per year- those who are currently enrolled, have never been to school, or have dropped out. In each rural district, 30 villages are sampled. In each village, 20 randomly selected households are surveyed. All the children in a selected household, in the age group 5-16 are administered the same tests in basic reading and basic arithmetic.[1]

The usual age at which students enroll in school is 5 or 6. I use children aged 5-16 to study the schooling and cognitive outcomes among the ineligible older siblings of eligible girls. The ineligible birth cohorts are from 1993-2005 and the exposed birth cohorts are from 2006-2009. I define ineligible sibling as just the first born before 2006 in a family with second born program eligible girl born in 2006 or later.

The survey asks every child in a household, aged 5 and above, four questions each in reading and math skills in their native or English language. For testing the math skills, each child is asked questions on four math tasks. The tasks are whether the child can recognize number 1-9; recognize numbers 10-99; can subtract; and can divide. The math score is coded as 1 if the child correctly completes the tasks and 0 otherwise.

[1] ASER surveyors conduct the surveys on Sundays, when most people are at home and children are not in school, and must return to households if the children are not present at the time of survey.

Then I calculate the "math score" which ranges from 0-4 and is the sum of scores of the four questions asked to every child. The four reading questions are whether the child can recognize letters; words; read a grade one text; and can read a grade two text. The reading scores are coded as 1 if the child correctly answers each question and 0 otherwise. Then I calculate the "read score" which ranges from 0-4 and is the sum of scores of the four reading questions. A read or math score of 0 implies that the child cannot read or do any math and the score keeps increasing by one as the child can correctly reads or solves the higher level of questions.

2.3.2 Estimating Equation

The objective of my empirical strategy is to assess the spillover effects of a conditional cash transfer program, Ladli Laxmi Yojana in an Indian state, Madhya Pradesh on educational outcomes of the ineligible siblings. I use a difference-in-differences framework to study the effects of the policy on the ineligible older sibling in a household with a program-eligible second born girl.

Since the program covered first and (or) second born girls born on or after January 1, 2006, I use the ineligible first born girls and boys born before 2006 with an eligible sister born in 2006 or later as the treated group, and the first born children born in households with no eligible sisters as the comparison group. That is, I define a treated child as the one who is a first born in a family with a second born program-eligible girl. Using the first born ineligible girls and boys before 2006 in MP, I employ a double difference by using the other five backward states, Chattisgarh, Jharkhand, Uttarakhand, Odisha and Rajasthan as the comparison group.

Thus, the difference-in-differences equation using the individual level data is:

$$Y_{ihjt} = \beta_0 + \beta_1 \cdot (MP_{ij} \cdot Eligiblehh_{ih}) + \beta_2 \cdot Eligiblehh_{ih} + \delta_j + \phi_t + \gamma_c + \mu X'_{ihj} + U_{ihjt} \quad (2.1)$$

Y_{ijh} is the outcome variable for first born child i in eligible household h in state j at time t. Since the policy was announced in 2007 for birth cohorts born from 2006, I define eligible households,$eligiblehh$, as the one that has a first born child born before 2006 and a second born girl born in 2006 or later. δ_j, $\phi_[t]$, and γ_c are the state, year, and cohort fixed effects. Sample includes birth cohorts born in years 1993-2005. X'_{ijc} is a vector of individual and household characteristics controls,which includes mother's age,and mother's education. β_1 is the double difference estimator that estimates the spillover effects of the policy on schooling and cognition outcomes of the ineligible children with eligible sisters. The results from this specification are reported in the appendix.

In the above specification, by definition the treatment group is just the ineligible first born children with a second born sister. On the other hand, the comparison group will have ineligible first born children with both second born sister and second born brother. The outcome variables can differ for the two group even without the program. Additionally, the difference-in-differences estimator will give the causal effect of the program if there were no differential trends in the outcomes, among MP and comparison states. The identification strategy can be used to get the cohort-by-cohort analysis for the outcome variables. Consider the following relationship between schooling outcomes of an ineligible first born i, born in state j, in year t, in eligible household h, and his/her exposure to the program:

$$Y_{ihjt} = \beta_0 + \beta_{1l} \cdot \sum_{l=1}^{17} (MP_{ij} \cdot yobsister_{it}) + \delta_j + \phi_t + \gamma_c + \mu X'_{ihj} + U_{ihjt} \tag{2.2}$$

where $yobsister_{it}$ is a dummy that indicates the year of the birth of the second born sister. The sample consists of ineligible first born children in families with program-eligible second born girls. Using the above specification, I measure the effect of exposure to the program based on the year-of-birth of the second born sister. I measure the cohort variation (by the second sister's year-of-birth) in schooling outcomes for the ineligible older sibling with 17 year-of-birth dummies. Second born sisters born in 1994 form the control group, and is omitted from the regression. Each coefficient β_{1n} gives an estimate of the impact of being an ineligible first born in a family with a second born girl. This shows the effect on the ineligible children before and after the program. Children in families with program-eligible sisters are exposed to the program as compared to the ones whose second born sister was born before the program. In order for the parallel trends assumption to hold, the coefficients should be zero or close to zero for the before cohorts. Given that I find evidence of differential trends in outcomes between the treated and comparison states, I add control for a linear trend using an interaction between the year-of-birth of the second born sister and the treated state, MP. I can rewrite specification (1) to control for the differential trends in outcomes as:

$$Y_{ihjt} = \beta_0 + \beta_1 \cdot (MP_{ij} \cdot Eligiblehh_{ih}) + \beta_2 \cdot Eligiblehh_{ih} + \beta_3 \cdot (MP_{ij} \cdot yobsister)$$

$$+\delta_j + \phi_t + \gamma_c + \mu X'_{ihj} + U_{ihjt} \tag{2.3}$$

where, $MP_{ij} \cdot yobsister$ is an interaction dummy between the year-of-birth of the second sister and the MP dummy. Adding this interaction linearly controls for the differential trends in the schooling and cognition outcome of the ineligible first born. Additionally, the comparison group in this specification only consists of first born children with a second born sister. This is the preferred specification and the results are presented in the next section.

Before I present my results in the next section, a few points must be taken into account. First, inference is based on robust standard errors. Second, I define treated ineligible siblings as just the first born in a family with a second born eligible sister. This is because the program was designed to also affect the fertility behavior of parents and the birth of the third and later borns in these families could have been affected by it.

Third, one of the eligibility conditions of LLY is that the family should be non- income tax payers. I do not enforce this condition when defining eligibility in my analyses. This is not a major concern as the number of income-tax payers in India is small due to several tax exemptions.[2] Moreover, tax evasion is widespread. For these reasons, it is unlikely that the income-tax status of a household is a strictly enforced or a binding condition for eligibility.

2.4 Results

In this section, I present the intent-to-treat reduced-form spillover effects of LLY on schooling and cognition outcomes on ineligible children. Tables 2-7 show results for the effect of exposure to the program on schooling outcomes using specification (3). Figures 1-6 plot the β_{1n} from specification (2). Each point on the graph is the coefficient of interaction between the treated state dummy and the year-of-birth of the second born sister. These gives us the relationship between the birth cohort of the second sister and the education outcomes of the first born children. I look at these separately for the first born sister and first born brother. For the schooling outcomes, these coefficients are increasing over time for both the highest grade completed and current grade for the first born sister but likelihood of being currently enrolled and ever enrolled is almost unchanged. For the older first born brother the effects are almost unchanged for all the schooling outcomes, but current grade. The reading and math test scores have a negative trend over the birth cohorts of the second sister, for both first born brother and first born sister. Given these pre-existing negative trends between MP and comparison states, I add a linear trend in the main specification to address the issue.

Tables 2 and 3 show the estimates of β_1 from specification(3) using the ASER data for schooling outcomes of both ineligible first born girl and boy, respectively. The birth

[2] Banerjee and Piketty (2005) show that incomes below the top 1 per cent are largely exempt from taxation in India.

cohorts of the ineligible first born are from 1993-2005. Column (1) shows the highest grade completed by a child. This is the highest grade of schooling completed by the child at the time of survey. It is zero if the child was never enrolled in schooled or if she dropped out in grade 1, and for those who are currently enrolled, it is the current grade minus 1. For the children who dropped out, highest grade completed is the grade in which they dropped out minus one. In Table 2, columns (1)-(4) suggest that exposure to the program did not have much effect on changing the schooling outcome of a first born sister in a family with a second born program-eligible sister. The results are similar for an ineligible first born brother. Although, for a family with a strong son preference, we would expect the results to be different for older brothers and older sisters, the results do not support the hypothesis.

The results in Tables 4-7 show the effect of the program on cognition skills of the ineligible first born children. The coefficient for the over all math score is negative for the ineligible older sister as compared to the positive coefficient for the ineligible older brother. However, given a scale of 0-4 for the scores, both these coefficients are relatively small and close to zero. The coefficient for reading scores is negative for both ineligible older sister and brother. The results remain negative even after controlling for the differential trend in scores between MP and comparison states, and are more

As a robustness check for the main results, I restrict my analyses to just 5-12 year old children. This limits the oldest age group in the treated group to just 12 years and is useful as the schooling effects can be quite different for much older children. I estimate specification (3) using first born children aged 5-12 and drop households with older first born children from the analyses. The results for this restricted age group are reported in Tables 8-13. One of the concerns using much older group of children is that the dropout rates are highest among the middle and higher secondary school going children. These are usually in the age group of 13-17, and therefore I drop them from the analyses to check if my main results are affected by this age group. My main results hold robust to excluding the older children from the analyses.

Therefore, I find suggestive evidence of no change in the schooling outcomes of the ineligible first born children. However, I find evidence of change in math and reading test scores. Comparing these results with the own-effect of the program on the test scores, there is evidence of a negative effect on parental investments in their ineligible children's education outcomes, in terms of cognition skills. One reason for these results is that in a credit constrained family, parents will try and divert resources towards the program-eligible girl. This reallocation can be in terms of investing more in their health, investing in terms of spending more time teaching and developing the skills of the program-eligible girl to make sure that she completes the different levels of schooling and eventually graduates from school. The results in the parallel study, (Jain, 2018) show that there is positive increase in the test scores of the program-eligible girls. Therefore, overall there is no evidence of spillover effects on the schooling outcomes of the ineligible children. However, there is evidence of a negative spillover effect on the cognition skills of the ineligible children, and is especially worse for the ineligible older sister.

2.5 Conclusion

This paper evaluates the spillover effect of a conditional cash transfer program that differs in design from most conventional CCT programs on the ineligible first born children in households with a program-eligible second born sister.

The results suggest that the incentive of getting a huge financial payment in future does not make parents reallocate resources away from the ineligible children. However, I do find evidence of a negative spillover effect of the program on test scores of these ineligible siblings. This is especially worse in case of an ineligible first born girl. It is therefore possible that the program along with having a positive effects on the education outcomes of the program-eligible girls, also had an unintended effect of inducing parents to reallocate some resources away from the ineligible children. This maybe particularly true among the credit constrained households where the parents would be forced to divert the available resources towards the human capital development of the program-eligible girl.

There can be several reasons for these effects of the program. First, for the credit constrained households to react positively to the program, parents maybe forced to re-allocate resources away from the ineligible siblings. Second, I am only able to look at the outcomes of children in rural areas and most rural areas have government schools. The cost of sending a child to a government school in monetary terms is minimal. Therefore, this might be a reason I do not find much evidence of change in schooling outcomes. However, it can be costly to invest in the cognitive skill development of the children and therefore, parents are induced to invest more in the program-eligible girl than in her siblings, especially an ineligible sister. This in turn can result in unintended negative spillover effect of the program on ineligible children. The program was designed to empower and improve the well-being of girls in the state, and negative spillover effects on the ineligible siblings will prove to be a impediment in achieving this objective. This study shows the short-run spillover effects of this program on the ineligible children. However, there can be difference in short run versus long run exposure to the program on the ineligible siblings. ASER is expected to continue these annual surveys and hence I expect to look at the long term exposure in the future.

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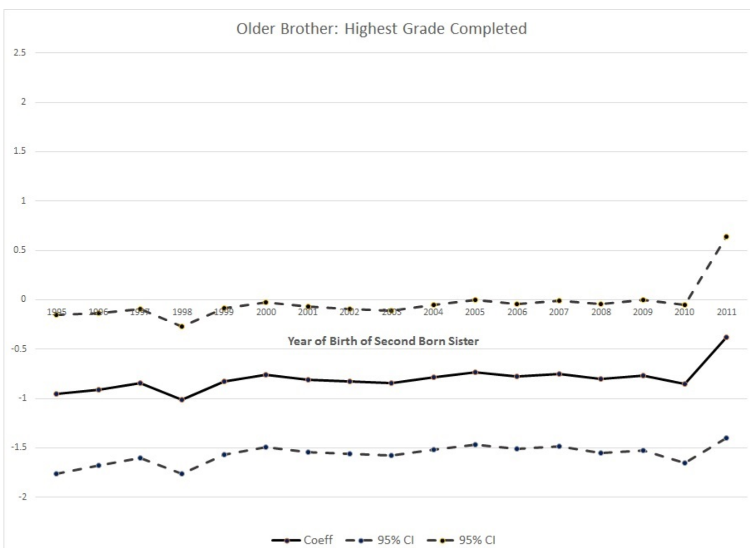
2.6 Figures

Figure 2.1: **Cohort-specific Effects: Highest Grade Completed by the Ineligible First Born**

(a) Highest Grade Completed by the Older Ineligible Sister

(b) Highest Grade Completed by the Older Ineligible Brother

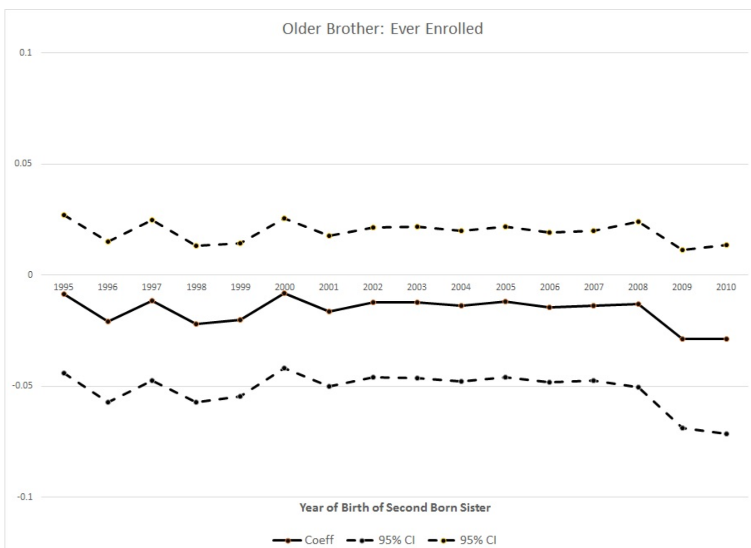


Notes: Author's calculation of highest grade completed using the ASER data. This figure plots the difference-in-differences coefficients of the interactions year-of-birth of the second born sister and Madhya Pradesh (MP) dummies from equation (2). Birth cohort 1994 is the omitted reference group from both the specifications.

Figure 2.2: **Cohort-specific Effects: Likelihood of Ever Enrolled by the Ineligible First Born**

(a) Likelihood of Ever Enrolled by the Older Ineligible Sister

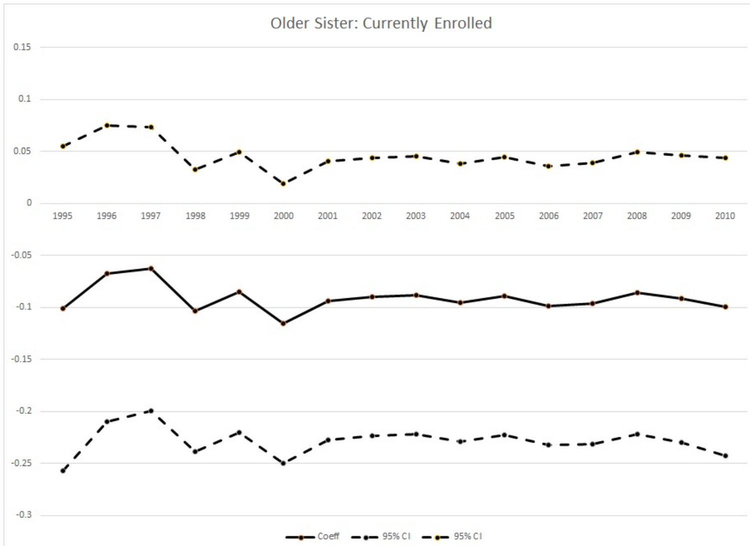
(b) Likelihood of Ever Enrolled by the Older Ineligible Brother



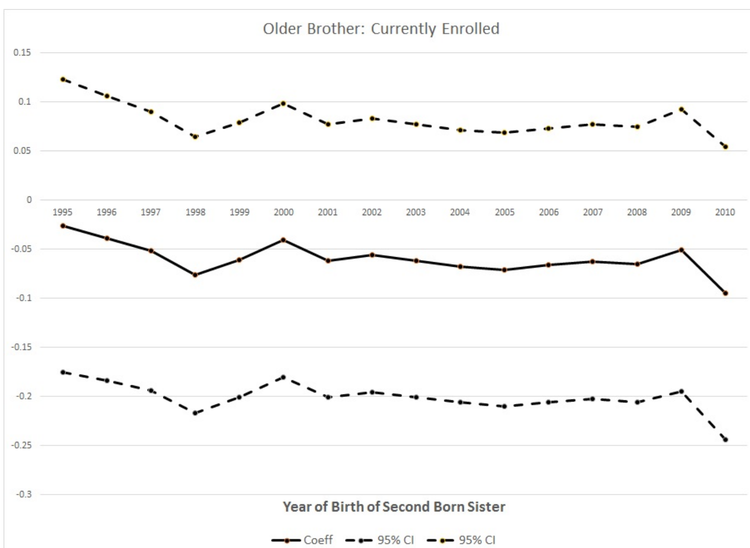
Notes: Author's calculation of likelihood of ever enrolled using the ASER data. This figure plots the difference-in-differences coefficients of the interactions year-of-birth of the second born sister and Madhya Pradesh (MP) dummies from equation (2). Birth cohort 1994 is the omitted reference group from both the specifications.

Figure 2.3: **Cohort-specific Effects: Likelihood of Currently Enrolled by the Ineligible First Born**

(a) Likelihood of Currently Enrolled by the Older Ineligible Sister



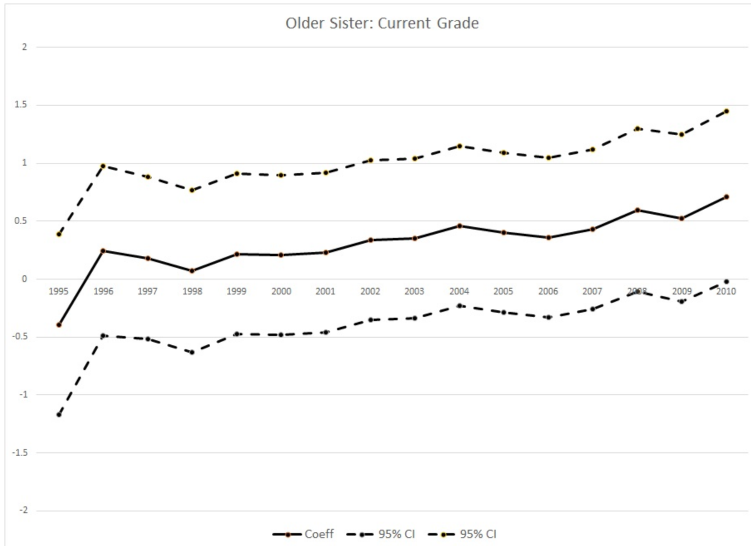
(b) Likelihood of Currently Enrolled by the Older Ineligible Brother



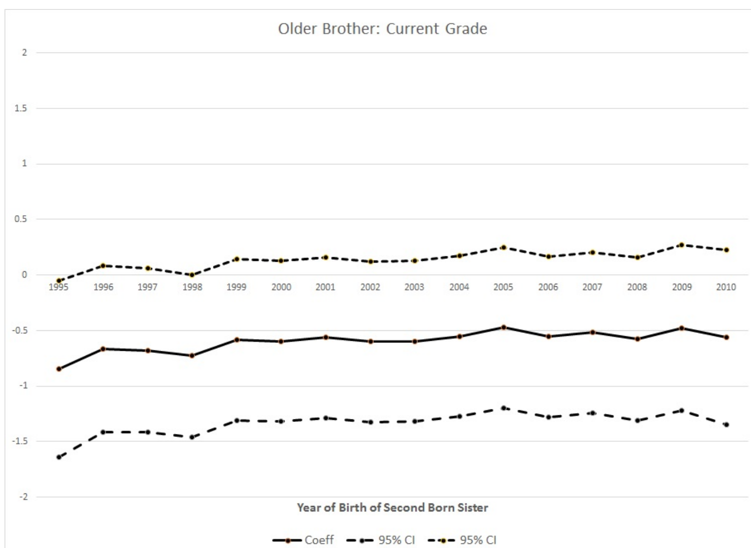
Notes: Author's calculation of likelihood of currently enrolled using the ASER data. This figure plots the difference-in-differences coefficients of the interactions year-of-birth of the second born sister and Madhya Pradesh (MP) dummies from equation (2). Birth cohort 1994 is the omitted reference group from both the specifications.

Figure 2.4: **Cohort-specific Effects: Current Grade of the Ineligible First Born**

(a) Current Grade of the Older Ineligible Sister



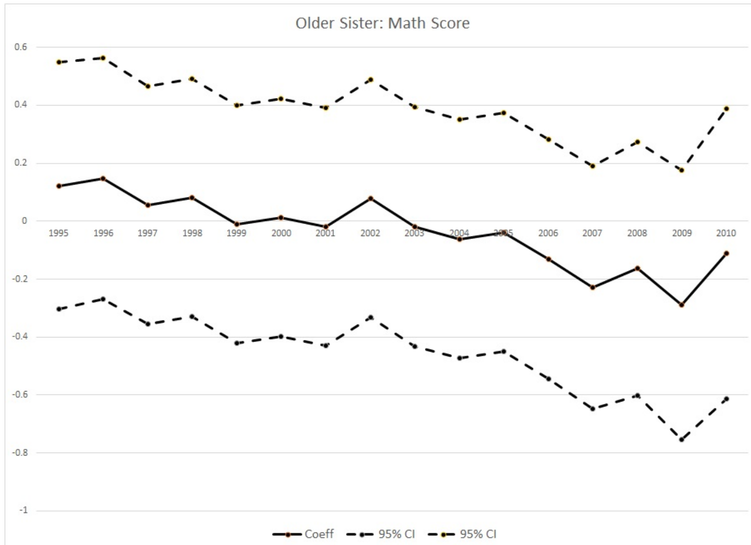
(b) Current Grade of the Older Ineligible Brother



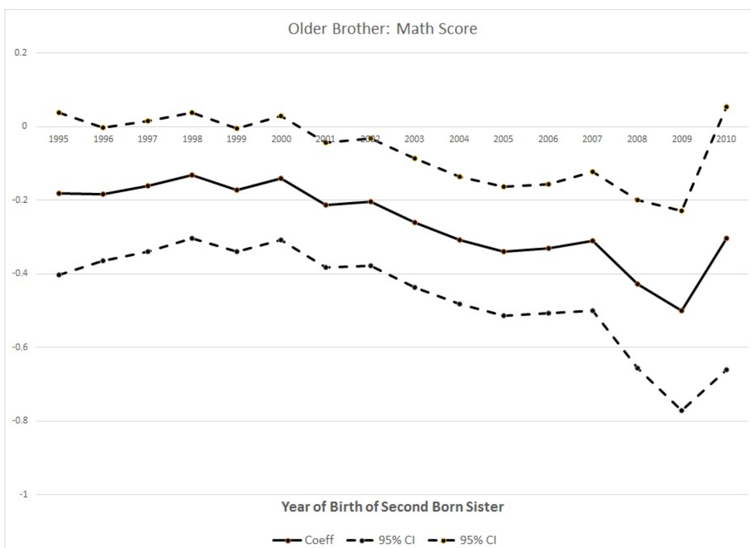
Notes: Author's calculation of the current grade using the ASER data. This figure plots the difference-in-differences coefficients of the interactions year-of-birth of the second born sister and Madhya Pradesh (MP) dummies from equation (2). Birth cohort 1994 is the omitted reference group from both the specifications.

Figure 2.5: **Cohort-specific Effects: Math Score of the Ineligible First Born**

(a) Math Score of the Older Ineligible Sister



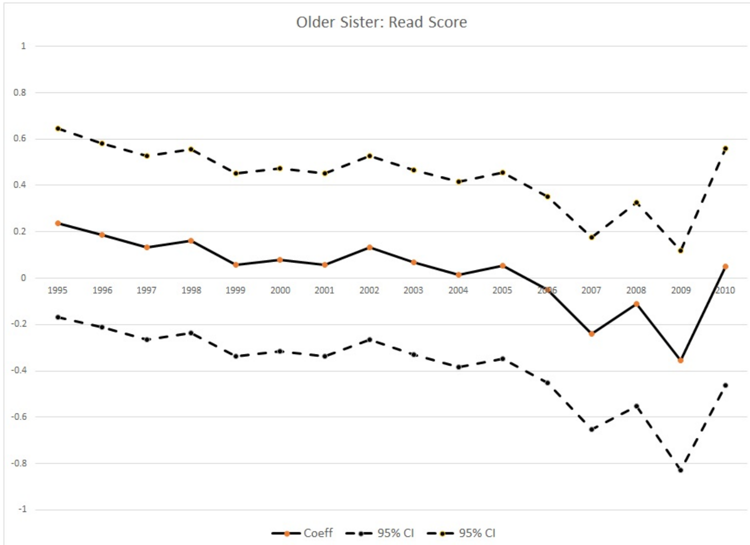
(b) Math Score of the Older Ineligible Brother



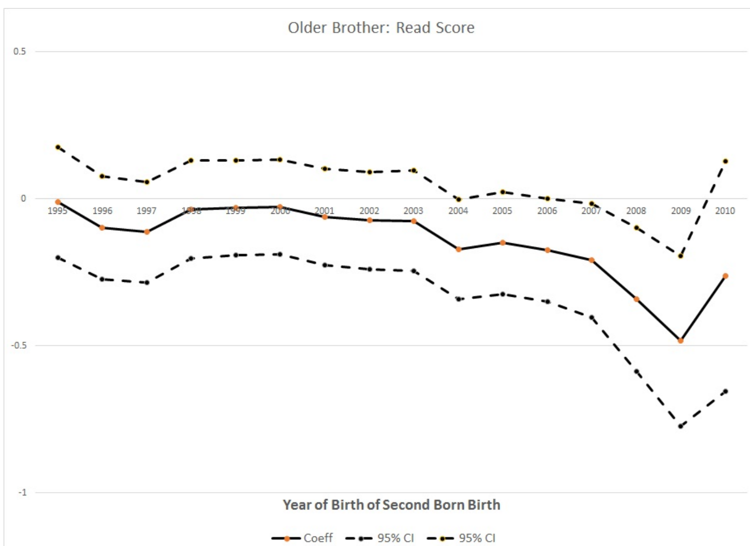
Notes: Author's calculation of the math score using the ASER data. This figure plots the difference-in-differences coefficients of the interactions year-of-birth of the second born sister and Madhya Pradesh (MP) dummies from equation (2). Birth cohort 1994 is the omitted reference group from both the specifications.

Figure 2.6: **Cohort-specific Effects: Read Score of the Ineligible First Born**

(a) Read Score of the Older Ineligible Sister



(b) Read Score of the Older Ineligible Brother



Notes: Author's calculation of the read score using the ASER data. This figure plots the difference-in-differences coefficients of the interactions year-of-birth of the second born sister and Madhya Pradesh (MP) dummies from equation (2). Birth cohort 1994 is the omitted reference group from both the specifications.

2.7 Tables

Table 2.1: **Summary statistics of key variables using ASER data**

	(1) MP	(2) Obs.	(3) Non-MP	(4) Obs.
	Panel A: Older Sister			
Highest Grade Completed	3.89	7583	3.97	3032
	(2.59)		(2.50)	
Ever Enrolled	0.98	7794	0.99	3108
	(0.14)		(0.11)	
Currently Enrolled	0.92	7794	0.93	3108
	(0.28)		(0.25)	
Current Grade	4.95	7147	4.98	2901
	(2.55)		(2.46)	
Math Score	2.33	6863	2.13	2753
	(1.27)		(1.26)	
Read Score	2.70	6882	2.48	2761
	(1.41)		(1.41)	
	Panel A: Older Brother			
Highest Grade Completed	4.08	8665	4.03	3670
	(2.67)		(2.64)	
Ever Enrolled	0.99	8935	0.99	3762
	(0.12)		(0.10)	
Currently Enrolled	0.93	8935	0.94	3762
	(0.26)		(0.24)	
Current Grade	5.11	8295	5.03	3533
	(2.64)		(2.61)	
Math Score	2.51	7655	2.41	3294
	(1.27)		(1.27)	
Read Score	2.78	7699	2.69	3307
	(1.36)		(1.37)	

Source: ASER 2009-2014

Notes: Summary statistics for schooling outcome and control variables in Madhya Pradesh and comparison states (Non-MP). Sample is the child-level data of first born children from birth cohorts 1993-2005 in households with program-eligible second born sister. Highest Grade Completed is the current grade minus 1 for those who are currently attending school and dropout grade minus 1 for those who dropped out. Also, if a child was never enrolled then, highest grade is coded as 0. Ever Enrolled is if the child was (is) enrolled in a school. Currently attending school is a dummy if the child is currently enrolled in school. Current grade is the grade in which the child is currently at.It is coded between 1 and 12. Math score ranges from 0-4 and is the sum of scores for the four math questions (scored 1 or 0)- nothing; numbers 1 to 9; numbers 10 to99; subtract and divide. Read score also ranges from 0-4 and is the sum of four reading questions- nothing; letter; word; grade 1 text and grade 2 text. Standard deviation is reported in parentheses.

Table 2.2: **Schooling outcomes of the ineligible older sister**

	(1) Highest grade completed	(2) Ever Enrolled	(3) Currently Enrolled	(4) Current Grade
Eligible hh*MP	0.0312	0.0202	0.00151	-0.00587
	(0.0597)	(0.0554)	(0.00269)	(0.00890)
Eligible hh	-0.226***	-0.218***	-0.00306*	0.00365
	(0.0291)	(0.0261)	(0.00168)	(0.00439)
mp*yobsister	0.0202***	0.0247***	-0.000302	0.000270
	(0.00727)	(0.00657)	(0.000317)	(0.00114)
Observations	24,918	23,840	25,304	25,304
State FE	Y	Y	Y	Y
Cohort FE	Y	Y	Y	Y
Year FE	Y	Y	Y	Y
Controls	Y	Y	Y	Y

Source: ASER 2009-2014
Notes: The table shows result for schooling outcomes for ineligible older sisters of the eligible second born.Controls include mother's age and education. Robust Standard errors are in parentheses. *** p<0.01, ** p<0.05, * p<0.1

Table 2.3: **Schooling outcomes of the ineligible older brother**

	(1) Highest grade completed	(2) Ever Enrolled	(3) Currently Enrolled	(4) Current Grade
Eligible hh*MP	0.0505	0.0233	-0.00348	0.00333
	(0.0554)	(0.0512)	(0.00256)	(0.00829)
Eligible hh	-0.194***	-0.186***	0.000709	0.000414
	(0.0275)	(0.0251)	(0.00158)	(0.00416)
mp*yobsister	-0.000213	0.00465	0.000318	-0.00145
	(0.00697)	(0.00615)	(0.000358)	(0.00104)
Observations	26,605	25,636	27,017	27,017
State FE	Y	Y	Y	Y
Cohort FE	Y	Y	Y	Y
Year FE	Y	Y	Y	Y
Controls	Y	Y	Y	Y

Source: ASER 2009-2014

Notes: The table shows result for schooling outcomes for ineligible older brothers of the eligible second born.Controls include mother's age and education. Robust Standard errors are in parentheses.

*** p<0.01, ** p<0.05, * p<0.1

Table 2.4: **Math test scores of the ineligible older sister**

	(1) Math Score	(2) Nothing	(3) Numbers 1to9	(4) Numbers 10to99	(5) Subtract	(6) Divide
Eligible hh*MP	-0.0698	0.0165*	0.0272*	-0.00329	-0.0714***	0.0309
	(0.0450)	(0.00862)	(0.0160)	(0.0197)	(0.0196)	(0.0192)
Eligible hh	-0.0123	-0.000922	0.0232***	-0.0124	-0.0290***	0.0190*
	(0.0218)	(0.00356)	(0.00676)	(0.00919)	(0.00949)	(0.00982)
mp*yobsister	-0.0198***	-0.000459	0.00253**	0.00546***	0.00311	-0.0106***
	(0.00431)	(0.000641)	(0.00123)	(0.00168)	(0.00200)	(0.00210)
Observations	22,730	22,730	22,730	22,730	22,730	22,730
State FE	Y	Y	Y	Y	Y	Y
Cohort FE	Y	Y	Y	Y	Y	Y
Year FE	Y	Y	Y	Y	Y	Y
Controls	Y	Y	Y	Y	Y	Y

Source: ASER 2009-2014

Notes: This table shows the results for the math test scores for ineligible sisters of eligible second born females. Controls include mother's age and education. Robust standard errors in parentheses. *** $p<0.01$, ** $p<0.05$, * $p<0.1$

Table 2.5: **Math test scores of the ineligible older brother**

	(1) Math Score	(2) Nothing	(3) Numbers 1to9	(4) Numbers 10to99	(5) Subtract	(6) Divide
Eligible hh*MP	0.00249	-0.00405	0.0366***	-0.0142	-0.0676***	0.0493***
	(0.0421)	(0.00786)	(0.0140)	(0.0181)	(0.0181)	(0.0184)
Eligible hh	-0.0337*	0.00896***	0.0105*	-0.000654	-0.0325***	0.0136
	(0.0203)	(0.00345)	(0.00607)	(0.00825)	(0.00892)	(0.00925)
mp*yobsister	-0.0231***	0.000521	0.00166	0.00706***	0.00187	-0.0111***
	(0.00407)	(0.000638)	(0.00107)	(0.00155)	(0.00183)	(0.00199)
Observations	23,917	23,917	23,917	23,917	23,917	23,917
State FE	Y	Y	Y	Y	Y	Y
Cohort FE	Y	Y	Y	Y	Y	Y
Year FE	Y	Y	Y	Y	Y	Y
Controls	Y	Y	Y	Y	Y	Y

Source: ASER 2009-2014

Notes: This table shows the results for the math test scores for ineligible brothers of eligible second born females. Controls include mother's age and education. Robust standard errors in parentheses.
*** p<0.01, ** p<0.05, * p<0.1

Table 2.6: **Read test scores of the ineligible older sister**

	(1) Read Score	(2) Nothing	(3) Letter	(4) Word	(5) Grade 1 Text	(6) Grade 2 Text
Eligible hh*MP	-0.132***	0.0104	0.0253*	0.0128	-0.0108	-0.0377*
	(0.0478)	(0.00846)	(0.0150)	(0.0150)	(0.0166)	(0.0197)
Eligible hh	-0.00194	-0.00268	0.0151**	-0.00552	-0.0217***	0.0148
	(0.0213)	(0.00363)	(0.00606)	(0.00668)	(0.00762)	(0.00929)
mp*yobsister	-0.0140***	-0.000460	0.00327***	0.00200*	0.00208	-0.00689***
	(0.00418)	(0.000675)	(0.00110)	(0.00117)	(0.00154)	(0.00188)
Observations	22,786	22,786	22,786	22,786	22,786	22,786
State FE	Y	Y	Y	Y	Y	Y
Cohort FE	Y	Y	Y	Y	Y	Y
Year FE	Y	Y	Y	Y	Y	Y
Controls	Y	Y	Y	Y	Y	Y

Source: ASER 2009-2014
Notes: This table shows the results for the read test scores for ineligible sisters of eligible second born females. Controls include mother's age and education. Robust standard errors in parentheses.
*** p<0.01, ** p<0.05, * p<0.1

Table 2.7: **Read test scores of the ineligible older brother**

	(1) Read Score	(2) Nothing	(3) Letter	(4) Word	(5) Grade 1 Text	(6) Grade 2 Text
Eligible hh*MP	-0.0689	0.000997	0.0355***	-0.00960	-0.0223	-0.00456
	(0.0431)	(0.00768)	(0.0133)	(0.0142)	(0.0160)	(0.0184)
Eligible hh	-0.0251	0.0121***	0.00470	-0.0136**	-0.0101	0.00695
	(0.0202)	(0.00336)	(0.00584)	(0.00654)	(0.00743)	(0.00888)
mp*yobsister	-0.0147***	4.37e-05	0.00149	0.00394***	0.00218	-0.00766***
	(0.00387)	(0.000623)	(0.000988)	(0.00118)	(0.00149)	(0.00180)
Observations	23,988	23,988	23,988	23,988	23,988	23,988
State FE	Y	Y	Y	Y	Y	Y
Cohort FE	Y	Y	Y	Y	Y	Y
Year FE	Y	Y	Y	Y	Y	Y
Controls	Y	Y	Y	Y	Y	Y

Source: ASER 2009-2014
Notes: This table shows the results for the read test scores for ineligible brothers of eligible second born females. Controls include mother's age and education. Robust standard errors in parentheses.
*** p<0.01, ** p<0.05, * p<0.1

Table 2.8: **Schooling outcomes of the ineligible older sister, aged 5-12**

	(1) Highest grade completed	(2) Ever Enrolled	(3) Currently Enrolled	(4) Current Grade
Eligible hh*MP	0.0734	0.0351	0.00304	-0.000101
	(0.0622)	(0.0591)	(0.00276)	(0.00961)
Eligible hh	-0.183***	-0.167***	-0.00279	-0.00502
	(0.0274)	(0.0252)	(0.00176)	(0.00450)
mp*yobsister	0.00820	0.0145*	-0.000157	0.000273
	(0.00923)	(0.00865)	(0.000343)	(0.00155)
Observations	17,842	17,337	18,140	18,140
State FE	Y	Y	Y	Y
Cohort FE	Y	Y	Y	Y
Year FE	Y	Y	Y	Y
Controls	Y	Y	Y	Y

Source: ASER 2009-2014
Notes: The table shows result for schooling outcomes for ineligible older sisters of the eligible second born.Controls include mother's age and education. Robust Standard errors are in parentheses. *** p<0.01, ** p<0.05, * p<0.1

Table 2.9: **Schooling outcomes of the ineligible older brother, aged 5-12**

	(1) Highest grade completed	(2) Ever Enrolled	(3) Currently Enrolled	(4) Current Grade
Eligible hh*MP	0.0660	0.0568	-0.00215	-0.00624
	(0.0588)	(0.0567)	(0.00245)	(0.00884)
Eligible hh	-0.165***	-0.171***	0.00242	0.000568
	(0.0276)	(0.0265)	(0.00151)	(0.00406)
mp*yobsister	-0.00827	-0.00271	-0.000376	0.000230
	(0.00911)	(0.00865)	(0.000394)	(0.00138)
Observations	18,547	18,150	18,852	18,852
State FE	Y	Y	Y	Y
Cohort FE	Y	Y	Y	Y
Year FE	Y	Y	Y	Y
Controls	Y	Y	Y	Y

Source: ASER 2009-2014
Notes: The table shows result for schooling outcomes for ineligible older brothers of the eligible second born.Controls include mother's age and education. Robust Standard errors are in parentheses. *** p<0.01, ** p<0.05, * p<0.1

Table 2.10: **Math test scores of the ineligible older sister, aged 5-12**

	(1) Math Score	(2) Nothing	(3) Numbers 1to9	(4) Numbers 10to99	(5) Subtract	(6) Divide
Eligible hh*MP	-0.0229	0.0201*	0.0148	-0.0126	-0.0769***	0.0545**
	(0.0554)	(0.0105)	(0.0197)	(0.0247)	(0.0241)	(0.0233)
Eligible hh	5.19e-05	-0.00268	0.0183**	-0.0152	-0.0138	0.0134
	(0.0248)	(0.00406)	(0.00782)	(0.0107)	(0.0108)	(0.0110)
mp*yobsister	-0.0320***	-0.00111	0.00535**	0.00792**	0.00457	-0.0167***
	(0.00779)	(0.00122)	(0.00232)	(0.00326)	(0.00354)	(0.00366)
Observations	16,068	16,068	16,068	16,068	16,068	16,068
State FE	Y	Y	Y	Y	Y	Y
Cohort FE	Y	Y	Y	Y	Y	Y
Year FE	Y	Y	Y	Y	Y	Y
Controls	Y	Y	Y	Y	Y	Y

Source: ASER 2009-2014
Notes: This table shows the results for the math test scores for ineligible sisters of eligible second born females. Controls include mother's age and education. Robust standard errors in parentheses.
*** p<0.01, ** p<0.05, * p<0.1

Table 2.11: **Math test scores of the ineligible older brother, aged 5-12**

	(1) Math Score	(2) Nothing	(3) Numbers 1to9	(4) Numbers 10to99	(5) Subtract	(6) Divide
Eligible hh*MP	0.0310	-0.00884	0.0393**	-0.0327	-0.0481**	0.0504**
	(0.0532)	(0.00985)	(0.0178)	(0.0236)	(0.0233)	(0.0233)
Eligible hh	-0.0419*	0.0103**	0.00805	-0.00106	-0.0211**	0.00391
	(0.0239)	(0.00418)	(0.00724)	(0.00988)	(0.0104)	(0.0107)
mp*yobsister	-0.0260***	0.00123	0.00186	0.00982***	-0.00417	-0.00874**
	(0.00764)	(0.00120)	(0.00213)	(0.00316)	(0.00354)	(0.00373)
Observations	16,339	16,339	16,339	16,339	16,339	16,339
State FE	Y	Y	Y	Y	Y	Y
Cohort FE	Y	Y	Y	Y	Y	Y
Year FE	Y	Y	Y	Y	Y	Y
Controls	Y	Y	Y	Y	Y	Y

Source: ASER 2009-2014
Notes: This table shows the results for the math test scores for ineligible brothers of eligible second born females. Controls include mother's age and education. Robust standard errors in parentheses.
*** p<0.01, ** p<0.05, * p<0.1

Table 2.12: **Read test scores of the ineligible older sister, aged 5-12**

	(1) Read Score	(2) Nothing	(3) Letter	(4) Word	(5) Grade 1 Text	(6) Grade 2 Text
Eligible hh*MP	-0.113*	0.0105	0.0181	0.00978	-0.00228	-0.0360
	(0.0594)	(0.0105)	(0.0185)	(0.0186)	(0.0206)	(0.0244)
Eligible hh	0.0160	-0.00461	0.0127*	-0.00736	-0.0209**	0.0202*
	(0.0246)	(0.00422)	(0.00702)	(0.00769)	(0.00877)	(0.0106)
mp*yobsister	-0.0192**	-0.000553	0.00545**	0.00273	-0.000347	-0.00727**
	(0.00780)	(0.00127)	(0.00215)	(0.00223)	(0.00279)	(0.00342)
Observations	16,103	16,103	16,103	16,103	16,103	16,103
State FE	Y	Y	Y	Y	Y	Y
Cohort FE	Y	Y	Y	Y	Y	Y
Year FE	Y	Y	Y	Y	Y	Y
Controls	Y	Y	Y	Y	Y	Y

Source: ASER 2009-2014
Notes: This table shows the results for the read test scores for ineligible sisters of eligible second born females. Controls include mother's age and education. Robust standard errors in parentheses.
*** p<0.01, ** p<0.05, * p<0.1

Table 2.13: **Read test scores of the ineligible older brother, aged 5-12**

	(1) Read Score	(2) Nothing	(3) Letter	(4) Word	(5) Grade 1 Text	(6) Grade 2 Text
Eligible hh*MP	-0.0186	-0.00516	0.0371**	-0.0277	-0.0167	0.0124
	(0.0543)	(0.00944)	(0.0169)	(0.0184)	(0.0206)	(0.0235)
Eligible hh	-0.0182	0.0131***	-0.000502	-0.0135*	-0.00577	0.00664
	(0.0238)	(0.00401)	(0.00695)	(0.00782)	(0.00874)	(0.0104)
mp*yobsister	-0.0280***	0.00131	0.00226	0.00745***	0.00107	-0.0121***
	(0.00727)	(0.00117)	(0.00196)	(0.00230)	(0.00288)	(0.00341)
Observations	16,397	16,397	16,397	16,397	16,397	16,397
State FE	Y	Y	Y	Y	Y	Y
Cohort FE	Y	Y	Y	Y	Y	Y
Year FE	Y	Y	Y	Y	Y	Y
Controls	Y	Y	Y	Y	Y	Y

Source: ASER 2009-2014
Notes: This table shows the results for the read test scores for ineligible brothers of eligible second born females. Controls include mother's age and education. Robust standard errors in parentheses.
*** p<0.01, ** p<0.05, * p<0.1

2.8 Appendix

Table A1: **Schooling outcomes of the ineligible older sister**

	(1) Highest grade completed	(2) Ever Enrolled	(3) Currently Enrolled	(4) Current Grade
Eligible hh*MP	0.126***	0.131***	-0.000778	0.000234
	(0.0404)	(0.0374)	(0.00202)	(0.00635)
Eligible hh	-0.145***	-0.137***	-0.00133	-0.00142
	(0.0224)	(0.0204)	(0.00136)	(0.00360)
Observations	55,569	53,415	56,634	56,634
State FE	Y	Y	Y	Y
Cohort FE	Y	Y	Y	Y
Year FE	Y	Y	Y	Y
Controls	Y	Y	Y	Y

Source: ASER 2009-2014
Notes: The table shows result for schooling outcomes for ineligible older sisters of the eligible second born.Controls include mother's age and education. Robust Standard errors are in parentheses. *** p<0.01, ** p<0.05, * p<0.1

Table A2: **Schooling outcomes of the ineligible older brother**

	(1) Highest grade completed	(2) Ever Enrolled	(3) Currently Enrolled	(4) Current Grade
Eligible hh*MP	0.00210	0.00533	-0.00108	-0.00262
	(0.0365)	(0.0348)	(0.00197)	(0.00584)
Eligible hh	-0.101***	-0.0948***	0.000583	-0.000159
	(0.0214)	(0.0198)	(0.00118)	(0.00343)
Observations	61,381	59,252	62,627	62,627
State FE	Y	Y	Y	Y
Cohort FE	Y	Y	Y	Y
Year FE	Y	Y	Y	Y
Controls	Y	Y	Y	Y

Source: ASER 2009-2014
Notes: The table shows result for schooling outcomes for ineligible older brothers of the eligible second born.Controls include mother's age and education. Robust Standard errors are in parentheses. *** p<0.01, ** p<0.05, * p<0.1

Table A3: **Math test scores of the ineligible older sister**

	(1) Math Score	(2) Nothing	(3) Numbers 1to9	(4) Numbers 10to99	(5) Subtract	(6) Divide
Eligible hh*MP	-0.126***	0.0136*	0.0307**	0.0118	-0.0441***	-0.0120
	(0.0333)	(0.00724)	(0.0131)	(0.0153)	(0.0139)	(0.0131)
Eligible hh	-0.0442**	-0.000899	0.0185***	0.00503	-0.0179**	-0.00479
	(0.0186)	(0.00349)	(0.00641)	(0.00799)	(0.00785)	(0.00777)
Observations	50,649	50,649	50,649	50,649	50,649	50,649
State FE	Y	Y	Y	Y	Y	Y
Cohort FE	Y	Y	Y	Y	Y	Y
Year FE	Y	Y	Y	Y	Y	Y
Controls	Y	Y	Y	Y	Y	Y

Source: ASER 2009-2014
Notes: This table shows the results for the math test scores for ineligible sisters of eligible second born females. Controls include mother's age and education. Robust standard errors in parentheses.
*** p<0.01, ** p<0.05, * p<0.1

Table A4: **Math test scores of the ineligible older brother**

	(1) Math Score	(2) Nothing	(3) Numbers 1to9	(4) Numbers 10to99	(5) Subtract	(6) Divide
Eligible hh*MP	-0.107***	-0.00172	0.0396***	0.0212	-0.0471***	-0.0119
	(0.0317)	(0.00667)	(0.0114)	(0.0139)	(0.0128)	(0.0126)
Eligible hh	-0.0255	0.00653*	0.00333	-0.00193	-0.00678	-0.00115
	(0.0176)	(0.00352)	(0.00578)	(0.00737)	(0.00743)	(0.00741)
Observations	54,877	54,877	54,877	54,877	54,877	54,877
State FE	Y	Y	Y	Y	Y	Y
Cohort FE	Y	Y	Y	Y	Y	Y
Year FE	Y	Y	Y	Y	Y	Y
Controls	Y	Y	Y	Y	Y	Y

Source: ASER 2009-2014
Notes: This table shows the results for the math test scores for ineligible brothers of eligible second born females. Controls include mother's age and education. Robust standard errors in parentheses.
*** p<0.01, ** p<0.05, * p<0.1

Table A5: **Read test scores of the ineligible older sister**

	(1) Read Score	(2) Nothing	(3) Letter	(4) Word	(5) Grade 1 Text	(6) Grade 2 Text
Eligible hh*MP	-0.160***	0.00598	0.0342***	0.0161	0.00109	-0.0574***
	(0.0371)	(0.00698)	(0.0124)	(0.0121)	(0.0122)	(0.0145)
Eligible hh	-0.0353*	-0.00241	0.0113*	0.00949	-0.00795	-0.0104
	(0.0190)	(0.00355)	(0.00579)	(0.00606)	(0.00638)	(0.00791)
Observations	50,788	50,788	50,788	50,788	50,788	50,788
State FE	Y	Y	Y	Y	Y	Y
Cohort FE	Y	Y	Y	Y	Y	Y
Year FE	Y	Y	Y	Y	Y	Y
Controls	Y	Y	Y	Y	Y	Y

Source: ASER 2009-2014
Notes: This table shows the results for the read test scores for ineligible sisters of eligible second born females. Controls include mother's age and education. Robust standard errors in parentheses.
*** p<0.01, ** p<0.05, * p<0.1

Table A6: **Read test scores of the ineligible older brother**

	(1) Read Score	(2) Nothing	(3) Letter	(4) Word	(5) Grade 1 Text	(6) Grade 2 Text
Eligible hh*MP	-0.117***	0.00126	0.0334***	0.00962	-0.00787	-0.0364***
	(0.0343)	(0.00680)	(0.0109)	(0.0111)	(0.0117)	(0.0135)
Eligible hh	-0.00850	0.00742**	-0.00465	-0.00320	-0.000838	0.00127
	(0.0184)	(0.00352)	(0.00553)	(0.00586)	(0.00640)	(0.00760)
Observations	55,012	55,012	55,012	55,012	55,012	55,012
State FE	Y	Y	Y	Y	Y	Y
Cohort FE	Y	Y	Y	Y	Y	Y
Year FE	Y	Y	Y	Y	Y	Y
Controls	Y	Y	Y	Y	Y	Y

Source: ASER 2009-2014
Notes: This table shows the results for the read test scores for ineligible brothers of eligible second born females. Controls include mother's age and education. Robust standard errors in parentheses.
*** p<0.01, ** p<0.05, * p<0.1

Appendix A

Data Appendix

A.1 Civil Registration System Data

The dataset is the official state panel data on registered births. I use data from 2001-2015. Given that the program was announced in April, 2007, I define After as birth cohorts born from 2007 onwards. This data has information on registered births for births that took place in the given year and were registered in that year itself. Therefore for a given year, there is no record of the births that took place in previous years and got registered in that year. Following is the list and description of variables I use in the analysis using this dataset.

Table A1: **Variable Description**

Variables	Description
Female Share	Authors calculation of share of female births in total registered births
Sex Ratio	Authors calculation of total female births divided by total male births

A.2 India Human Development Survey, 2011-12

The dataset is a household survey data for year 2011-12. It has information on child birth history of all the surveyed ever married women. I use information on gender of the children- both the ones that are alive and those who had died by the time of the survey. This helps me generate sex ratio at birth for all the births. These are also- both registered and unregistered births. For the fertility preference of the parents I use information on the total number of children women have by the time of survey and from what year did they start having children. I also have information on the gender of the children. Finally, I also use information on sterilization. For the fertility outcomes I am only including women with at leaat one child by the time of survey.

Table A2: **Variable Description**

Variables	Description
Sex ratio at birth	Ratio of female to male child birth- both those who died and were alive by the time of survey
Eligible Case1 Woman	dummy variable: is a woman who has no children by the year 2005
Eligible Case2 woman	dummy variable: is a woman who has one children by the year 2005
Sterilization	dummy variable: either the woman or her husband has adopted sterilization as a method of family planning by the time of survey

A.3 Annual Status of Education Report, 2009-14

This is a household survey of yearly cross-section data from 2009 to 2014. This survey is conducted only in the rural areas and has information on schooling outcomes and math and reading test scores of children. In a surveyed household, every child aged 3-16 is included in the survey. Every child aged 5-16 is surveyed and even those who are not in school, i.e, they either dropped out or were never enrolled in the school are also included.

Table A3: **Variable Description**

Variables	Description
Highest Grade Completed	is the current grade minus 1 for those who are currently attending school and dropout grade minus 1 for those who dropped out. Also, if a child was never enrolled then, highest grade is coded as 0.
Current Grade	dummy variable: only defined for the ones who are currently enrolled in a grade in school
Ever Enrolled	dummy variable:if the child was ever enrolled in the school
Currently Enrolled	dummy variable: if the child is enrolled in the school
Math Score	ranges from 0-4 and is the sum of scores for the four math questions (scored 1 or 0)- nothing; numbers 1 to 9; numbers 10 to99; subtract and divide.
Read score	ranges from 0-4 and is the sum of four reading questions (scored 0 or 1)- nothing; letter; word; grade 1 text and grade 2 text.